37

PROCEEDINGS

OF

THE AMERICAN ASSOCIATION

FOR THE

ADVANCEMENT OF SCIENCE.

NINTH MEETING,

HELD AT PROVIDENCE, R. I.,

AUGUST, 1855.

CAMBRIDGE:
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1856.

EDITED BY

JOSEPH LOVERING,

Permanent Secretary.

CAMBRIDGE:
METCALF AND COMPANY, PRINTERS TO THE UNIVERSITY.

TO

CITIZENS OF PROVIDENCE,

BY WHOSE HOSPITALITY THE ASSOCIATION WERE ENTERTAINED,

AND

TO WHOSE LIBERALITY THEY ARE INDEBTED FOR ASSISTANCE
IN THE MEANS OF PUBLICATION,

THIS VOLUME OF PROCEEDINGS

IS RESPECTFULLY INSCRIBED.

TABLE OF CONTENTS.

COMMUNICATIONS.

A. MATHEMATICS AND PHYSICS.

I. MATHEMATICS.

II. ASTRONOMY.

III. PHYSICS.

IV. PHYSICS OF THE GLOBE.

V. METEOROLOGY.

B. CHEMISTRY AND NATURAL HISTORY.

I. CHEMISTRY.

II. MINERALOGY.

III. GEOLOGY.

IV. BOTANY.

V. PHYSIOLOGY.

C. PRACTICAL SCIENCE.

ENGINEERING.

EXECUTIVE PROCEEDINGS.

OFFICERS OF THE ASSOCIATION

AT THE

PROVIDENCE MEETING.

Prof. John Torrey, *President.*
Prof. Joseph Lovering, *Permanent Secretary.*
Prof. Wolcott Gibbs, *General Secretary.*
Dr. A. L. Elwyn, *Treasurer.*

Standing Committee.

Prof. John Torrey,
Prof. Joseph Lovering,
Prof. Wolcott Gibbs,
Dr. A. L. Elwyn,
Prof. J. D. Dana,
Prof. J. L. Smith,
Prof. A. D. Bache,
Prof. Alexis Caswell,
Zachariah Allen, Esq.,
Prof. James Robb,
Dr. B. A. Gould, Jr.,
William Mitchell, Esq.

Local Committee.

Prof. Alexis Caswell, *Chairman.*
W. F. Webster, *Secretary.*

Zachariah Allen,
Dr. J. Mauran,
Thomas P. Shepard,
Stephen T. Olney,
Owen Mason,
Prof. G. I. Chace,
George Thurber,
James Y. Smith,
Thomas Davis,
John Kingsbury,
Thomas L. Dunnell,
Amos D. Smith,
Walter Manton,
William P. Bullock,
Robert H. Ives,
William Goddard,

Thomas C. Hoppin,
William W. Hoppin,
Dr. J. Hartshorn,
Dr. Grosvenor,
P. Allen, Jr.,
John C. Brown,
Richard J. Arnold,
Henry B. Anthony,
Francis Wayland,
Jacob Dunnell,
Dr. S. B. Tobey,
Dr. I. Ray,
Samuel Ames,
John Gorham,
H. L. Kendall,
William W. Brown,
Stephen H. Smith,
Nicholas Brown,
William G. Angell,
Moses B. Ives,
Thomas J. Stead,
Isaac Thurber,
Rufus Waterman,
Richard Waterman,
George H. Corliss.

SPECIAL COMMITTEES.

A. Committees continued from Former Meetings.

Committee to Memorialize the Legislature of Ohio on the Subject of a Geological Exploration of that State.

Judge Lane, Sandusky, *Chairman.*
John Andrews, Esq., Columbus.
S. Medary, Esq., Columbus.
Judge Vance, Hamilton.
John H. James, Esq., Urbana.
Prof. S. St. John, Hudson.
Robert Buchanan, Esq., Cinc'ti.
John P. Foote, Esq., Cincinnati.
Hon. Allen Trimble, Highl'd Co.
Hon. S. J. Andrews, Cleveland.

Committee to Revise the Constitution.

Prof. A. D. Bache,
Prof. J. Lawrence Smith,
Prof. John Leconte,
Dr. Wolcott Gibbs,
Dr. B. A. Gould, Jr.,
Prof. W. B. Rogers,
Prof. J. D. Dana,
Dr. Joseph Leidy,
Prof. S. S. Haldeman,
Dr. A. A. Gould.

Committee to Report in Relation to Uniform Standards in Weights, Measures, and Coinage.

Prof. A. D. Bache,
Prof. Joseph Henry,
Prof. J. H. Alexander,
Prof. John F. Frazer,
Prof. Wolcott Gibbs,
Prof. Benjamin Peirce,
Prof. John Leconte,
Prof. W. B. Rogers,
Dr. J. H. Gibbon,
Dr. B. A. Gould, Jr.,
Prof. J. Lawrence Smith.

Committee to Audit the Accounts of the Treasurer.

Dr. Franklin Bache,
William S. Vaux, Esq.

B. New Committees.

Consulting Committee on the Publication of the Providence Proceedings.

Prof. Benjamin Peirce,
Prof. Joseph Winlock,
Prof. Louis Agassiz,
Dr. Asa Gray.

Consulting Committee on the Republication of the Cleveland Volume.

Prof. Benjamin Peirce,
Prof. J. D. Dana,
Prof. Wolcott Gibbs,
Prof. A. D. Bache.

Committee to Memorialize the Legislature of New York in Reference to Fish-Breeding.

Prof. Louis Agassiz,
Prof. J. D. Dana.

OFFICERS OF THE ALBANY MEETING.

Prof. James Hall, *President.*
Prof. Joseph Lovering, *Permanent Secretary.*
Dr. B. A. Gould, Jr., *General Secretary.*
Dr. A. L. Elwyn, *Treasurer.*

Standing Committee.

Prof. James Hall,
Prof. Joseph Lovering,
Dr. B. A. Gould, Jr.,
Dr. A. L. Elwyn,
Prof. John Torrey,
Prof. Wolcott Gibbs.

Local Committee.

Dr. T. R. Beck, *Chairman.*
Dr. James H. Armsby, *Secretary.*

Erastus Corning, Esq.,
Gideon Hawley, Esq.,
Thomas W. Olcott, Esq.,
Hon. D. D. Barnard,
Hon. A. J. Parker,
Ezra P. Prentice,
J. V. L. Pruyn, Esq.,
Dr. Thomas Hun.

MEETINGS OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

	Place.	Date.	President.	General Secretary.	Permanent Sec'y.	Treasurer.
1st Meeting,	Philadelphia, Pa.	September 20, 1848,	W. C. Redfield, Esq.,	Prof. Walter R. Johnson,	. . .	Prof. J. Wyman.
2d "	Cambridge, Mass.	August 14, 1849,	Prof. Joseph Henry,	Prof. E. N. Horsford,	. . .	Dr. A. L. Elwyn.
3d "	Charleston, S. C.	March 12, 1850,	Prof. A. D. Bache,*	Prof. L. R. Gibbes,*	. . .	Dr. St. J. Ravenel.*
4th "	New Haven, Ct.	August 19, 1850,	Prof. A. D. Bache,	E. C. Herrick, Esq.,	. . .	Dr. A. L. Elwyn.
5th "	Cincinnati, Ohio,	May 5, 1851,	Prof. A. D. Bache,	Prof. W. B. Rogers,	Prof. S. F. Baird,	Dr. A. L. Elwyn.
6th "	Albany, N. Y.	August 19, 1851,	Prof. L. Agassiz,	Prof. W. B. Rogers,	Prof. S. F. Baird,	Dr. A. L. Elwyn.
7th "	Cleveland, Ohio,	July 28, 1853,	Prof. Benj. Peirce,	Prof. J. D. Dana,	Prof. S. F. Baird,	Dr. A. L. Elwyn.
8th "	Washington, D. C.	April 26, 1854,	Prof. J. D. Dana,	Prof. J. Lawrence Smith,	Prof. J. Lovering,	Dr. J. L. Le Conte.*
9th "	Providence, R. I.	August 15, 1855,	Prof. John Torrey,	Prof. Wolcott Gibbs,	Prof. J. Lovering,	Dr. A. L. Elwyn.

* In the absence of the regular officer.

CONSTITUTION OF THE ASSOCIATION.

OBJECTS.

The Society shall be called "The American Association for the Advancement of Science." The objects of the Association are, by periodical and migratory meetings, to promote intercourse between those who are cultivating science in different parts of the United States; to give a stronger and more general impulse, and a more systematic direction, to scientific research in our country; and to procure for the labors of scientific men increased facilities and a wider usefulness.

RULES.

MEMBERS.

Rule 1. Those persons whose names have been already enrolled in the published proceedings of the Association, and all those who have been invited to attend the meetings, shall be considered members, on subscribing to these rules.

Rule 2. Members of scientific societies, or learned bodies, having in view any of the objects of this Society, and publishing transactions, shall likewise be considered members, on subscribing to these rules.

Rule 3. Collegiate professors of natural history, physics, chemistry, mathematics, and political economy, and of the theoretical and applied sciences generally, also civil engineers and architects who have been employed in the construction or superintendence of public works, may become members, on subscribing to these rules.

Rule 4. Persons not embraced in the above provisions may become members of the Association, upon nomination by the Standing Committee, and by a majority of the members present.

OFFICERS.

Rule 5. The officers of the Association shall be a President, a Secretary, and a Treasurer; who shall be elected at each annual meeting, for the meeting of the ensuing year.

MEETINGS.

Rule 6. The Association shall meet annually, for one week or longer, the time and place of each meeting being determined by a vote of the Association at the previous meeting; and the arrangements for it shall be intrusted to the officers and the Local Committee.

STANDING COMMITTEE.

Rule 7. There shall be a Standing Committee, to consist of the President, Secretary, and Treasurer of the Association; the officers of the preceding year; the chairmen and secretaries of the Sections, after these shall have been organized; and six other members present, who shall have attended any of the previous meetings: to be elected by ballot.

Rule 8. The Committee, whose duty it shall be to manage the general business of the Association, shall sit during the meeting, and at any time in the interval between it and the next meeting, as the interests of the Association may require. It shall also be the duty of this Committee to nominate the general officers of the Association for the following year, and persons for admission to membership.

SECTIONS.

Rule 9. The Standing Committee shall organize the Society into Sections, permitting the number and scope of these Sections to vary in conformity to the wishes and the scientific business of the Association.

Rule 10. It shall be the duty of the Standing Committee, if, at any time, two or more Sections, induced by a deficiency of scientific communications, or by other means, request to be united into one, — or if at any time a single Section, overloaded with business, asks to be subdivided, — to effect the change, and generally to readjust the subdivisions of the Association, whenever, upon due representation, it promises to expedite the proceedings, and advance the purposes of the meeting.

SECTIONAL COMMITTEES AND OFFICERS.

Rule 11. Each Section shall appoint its own chairman and secre tary of the meeting; and it shall likewise have a standing committee, of such size as the Section may prefer. The secretaries of the Sections may appoint assistants, whenever, in the discharge of their duties, it becomes expedient.

Rule 12. It shall be the duty of the standing committee of each Section, assisted by the chairman, to arrange and direct the proceedings in their Section, to ascertain what written and oral communications are offered, and, for the better forwarding the business, to assign the order in which these communications shall appear, and the amount of time which each shall occupy; and it shall be the duty of the chairman to enforce these decisions of the committee.

These Sectional Committees shall likewise recommend subjects for systematic investigation, by members willing to undertake the researches, and present their results at the next annual meeting.

The committees shall likewise recommend reports on particular topics and departments of science, to be drawn up as occasion permits, by competent persons, and presented at subsequent annual meetings.

REPORTS OF PROCEEDINGS.

RULE 13. Whenever practicable, the proceedings shall be reported by professional reporters or stenographers, whose reports are to be revised by the secretaries before they appear in print.

PAPERS AND COMMUNICATIONS.

RULE 14. The author of any paper or communication shall be at liberty to retain his right of property therein, provided he declares such to be his wish before presenting it to the Society.

GENERAL AND EVENING MEETINGS.

RULE 15. At least three evenings of the week shall be reserved for general meetings of the Association, and the Standing Committee shall appoint these and any other general meetings which the objects and interests of the Association may call for.

These general meetings may, when convened for that purpose, give their attention to any topics of science which would otherwise come before the Section; and thus all the Sections may, for a longer or shorter time, reunite themselves to hear and consider any communications, or transact any business.

It shall be a part of the business of these General Meetings to receive the Address of the President of the last Annual Meeting; to hear such reports on scientific subjects as, from their general importance and interest, the Standing Committee shall select; also to receive from the chairmen of the Sections abstracts of the proceedings of their respective Sections; and to listen to communications and lectures explanatory of new and important discoveries and researches in science, and new inventions and processes in the arts.

ORDER OF PROCEEDINGS IN ORGANIZING A MEETING.

RULE 16. The Association shall be organized by the President of the preceding Annual Meeting. The question of the most eligible distribution of the Society into Sections shall then occupy the attention of the Association; when, a sufficient expression of opinion being procured, the meeting may adjourn; and the Standing Committee shall immediately proceed to divide the Association into Sections, and to allot to the Sections their general places of meeting. The Sections may then organize by electing their officers, and proceed to transact scientific and other business.

LOCAL COMMITTEE.

RULE 17. The Standing Committee shall appoint a Local Committee from among members residing at or near the place of meeting for the ensuing year; and it shall be the duty of the Local Committee, assisted by the officers, to make arrangements for the meeting.

SUBSCRIPTIONS.

RULE 18. The amount of the annual subscription of each member of the Association shall be two dollars; and one dollar in addition shall entitle him to a copy of the proceedings of the annual meeting. The members attending an annual meeting shall pay, on registering their names, an additional assessment of ——— dollars. These subscriptions to be received by the Treasurer or Secretary.

RULE 19. The names of all persons two years and more in arrears for annual dues, shall be erased from the list of members: provided that two notices of indebtedness, at an interval of at least three months, shall have previously been given.

ACCOUNTS.

RULE 20. The accounts of the Association shall be audited annually, by auditors appointed at each meeting.

ALTERATIONS OF THE CONSTITUTION.

RULE 21. No article of this Constitution shall be altered or amended, without the concurrence of three fourths of the members present, nor unless notice of the proposed amendment or alteration shall have been given at the preceding annual meeting.

RESOLUTIONS AND ENACTMENTS

OF A PERMANENT AND PROSPECTIVE CHARACTER,

PASSED PREVIOUS TO THE EIGHTH MEETING.

Resolved, That copies or abstracts of all communications made, either to the General Association or to the Sections, must be furnished by the authors; otherwise, only the titles shall appear in the published proceedings. (*Proceedings First Meeting*, 1848, p. 133.)

Resolved, That 1,000 copies of the Proceedings of the Association be published in pamphlet form, and placed at the disposal of the chairman of the Committee on Publication.

(*Proceedings First Meeting*, 1848, p. 133.)

Resolved, That a *manual* or *manuals of scientific observation and research*, especially adapted to the use of the American inquirer, comprising directions for properly observing phenomena in every department of physical science, and for making collections in natural history, etc., whether on land or at sea, is much needed at the present time; and that such a publication, placed in the hands of officers of the army and navy, would greatly tend to develop the natural resources of our extended country, and to the general advancement of science.

Resolved, That the American Association for the Advancement of Science cordially recommends the Smithsonian Institution to undertake the preparation of such a volume, under the editorial superintendence of its Secretary, to be published in its series of reports.

Resolved, That this Association will cordially co-operate in the production of such a manual or manuals, in whatever manner may be best adapted to secure the end in view.

(*Proceedings Second Meeting*, 1849, pp. 273, 351.)

Resolved, That no paper be read before the future meetings of the Association, unless an abstract of it has previously been presented to the Secretary.

Resolved, That hereafter all books, charts, maps, and specimens, which may be presented to the Association, shall be given to the Smithsonian Institution.

(*Proceedings Second Meeting*, 1849, p. 272.)

Resolved, That a Secretary be appointed, who shall hold his office for the term of three years, and shall have a salary of $300 per annum, and whose duty it shall be to compile for publication all proceedings or transactions of the Association, to superintend the publication of the same, and to conduct the correspondence; the title of said officer to be that of Permanent Secretary of the American Association. (*Proceedings Fourth Meeting*, 1850, p. 16.)

Resolved, That the Standing Committee have power to fix the duties of the Permanent Secretary of this Association.

Resolved, That the Permanent Secretary be a member, *ex officio*, of the Standing Committee.

Resolved, That the Permanent Secretary be instructed to erase from the list of members of this Association the names of all who, by the return of the Treasurer, shall appear to be two years in arrears for annual dues; suitable notice being given by two circulars from the Treasurer, at an interval of three months, to all who may fall within the intent of this resolution.

Resolved, That the Standing Committee have full power to complete and finish any outstanding business of the Association, in their name. (*Proceedings Fourth Meeting*, 1850, p. 341.)

Resolved, That a copy of the printed volume of Proceedings of the Meetings at Philadelphia, Cambridge, and New Haven be presented to the libraries of Harvard and Yale.

(*Proceedings Fourth Meeting*, 1850, p. 346.)

Resolved, That the Treasurer be requested to retain $300 of the funds in his hands, and belonging to the Association, for the purpose of paying the salary of the Permanent Secretary; said payment to be made at such time, and in such manner, as may be agreed upon by the Treasurer and Permanent Secretary.

Resolved, That copies of the Proceedings of the American Association be presented to the New York Lyceum and the Philadelphia Academy of Natural Sciences.

Resolved, That the Permanent Secretary be requested to provide minute-books, suitably ruled, for the list of members and titles of papers, minutes of the general and sectional meetings, and for the other purposes indicated in the rules.

Resolved, That whenever the Permanent Secretary notices any error of fact or unnecessary repetition, or any other important defect in the papers communicated for publication in the Proceedings of the Association, that he be authorized to commit the same to the author, or to the proper sub-committee of the Standing Committee, for correction. (*Proceedings Fourth Meeting*, 1850, pp. 390, 391.)

Resolved, That copies of the Proceedings of the American Association for the Advancement of Science be presented to the American Academy of Arts and Sciences, Boston; to the Boston Society of Natural History; to the New York Lyceum of Natural History; to the American Philosophical Society and to the Academy of Natural Sciences of Philadelphia; to the Smithsonian Institution; and to the Western Academy of Natural Sciences at Cincinnati.

(*Proceedings Fifth Meeting*, 1851, p. 249.)

Resolved, That the Standing Committees of the Sections be requested, before the close of the meeting, to present to the Permanent Secretary a list of the papers which have been read in the Sections, and which they desire to have published.

Resolved, That hereafter all members of this Association are particularly desired to forward to the Permanent Secretary, so as to be received before the day appointed for the Association to convene, complete titles of all the papers which they expect to present during its meeting, with an estimate of the time required for reading each, and such abstracts of their contents as may give a general idea of their nature.

Resolved, That the foregoing resolution form part of the Circular.

(*Proceedings Sixth Meeting*, 1852, p. 402.)

Resolved, That the annual fee of membership be $2.00; and that payment of an additional dollar entitle a member to a copy of the Proceedings of the Meeting.

Resolved, That all members of the Association who have not paid their dues, after the issue of two circulars at intervals of three months, notifying them of that fact, be stricken from the roll by the Permanent Secretary.

(*Proceedings Sixth Meeting*, 1852, p. 402.)

Whereas the provision of the Constitution appears to be indefinite in regard to the term of service of the chairmen and secretaries of the Sections, —

Resolved, That the Sections be requested to direct the chairmen of their several Standing Committees to attend to the current business of the Section, and to appoint a chairman for each day of the meeting.

Resolved, That the Sections be requested to appoint a secretary for the period of the meeting, whose duty it shall be to furnish to the Permanent Secretary, for publication, a full report of all proceedings and discussions.

Resolved, That the following resolutions be presented to the Association at the opening of the Cleveland Meeting, for adoption: —

"1. That all papers, either at the general or in the several sectional meetings, shall be read in the order in which they are entered upon the books of the Association; except that those which may be entered by a member of the Standing Committee of the Association shall be liable to postponement by the Standing Committees of the Sections."

If this regulation shall be adopted by the Association, members will recognize the expediency of entering the titles of their communications at as early a date as possible.

"2. That, if any communication should not be ready at the assigned time, it shall be dropped to the bottom of the list, and shall not be entitled to take precedence of any subsequent communication.

"3. That no exchanges shall be made between members, without authority of the respective Standing Committees."

(*Proceedings Sixth Meeting*, 1852, p. 405.)

Resolved, That the names of those only shall be entered in the list of members who shall have signified their acceptance.

(*Proceedings Seventh Meeting*, 1853.)

Resolved, That a sum not exceeding seventy-five dollars shall be paid to the Permanent Secretary, to defray the expenses necessary for attending each meeting of the Association.

(*Proceedings Seventh Meeting*, 1853.)

1st. *Resolved*, That priority of entry of a paper shall, as far as practicable, give precedence in its presentation; cases of exception to be decided by the Standing Committees of the Sections.

2d. *Resolved*, That, if any communication be not ready at the assigned time, it shall be dropped to the bottom of the list, and shall not be entitled to take precedence of any subsequent communication.

3d. *Resolved*, That no exchange shall be made between members without authority of the respective Standing Committees.

(*Proceedings Seventh Meeting*, 1853.)

Resolved, That the following members be requested to report on the subjects respectively assigned to them, viz.: —

PROF. A. D. BACHE. *On Recent Additions to our Knowledge of the Theory of Tides.*

PROF. JOSEPH HENRY. *On Recent Additions to our Knowledge of the Laws of Atmospheric Electricity.*

PROF. JAMES HALL. *On Recent Additions to our Knowledge of Paleozoic Rocks.*

PROF. J. L. SMITH. *On the Recent Progress of Micro-Chemistry.*

PROF. WOLCOTT GIBBS. *On the Recent Progress of Organic Chemistry.* (This report was made at the Providence Meeting.)

DR. JOSEPH LEIDY. *On the Remains of Fossil Reptiles and Mammals in North America.*

PROF. BENJAMIN PEIRCE. *On the Present State of the Theory of Planetary Perturbations.*

DR. W. I. BURNETT. *On Recent Advances in Anatomy and Physiology.*

PROF. LOUIS AGASSIZ. *On the History of our Knowledge of Alternation of Generation in Animals.*

PROF. J. D. DANA. *On the Geographical Distribution of the Lower Animals.*

PROF. L. S. HALDEMAN. *On the Present State of our Knowledge of Linguistic Ethnology.*

DR. B. A. GOULD, JR. *On the Progress and Developments of the Electro-Chronographic Method of Observation.*

(*Proceedings Seventh Meeting*, 1853.)

Resolved, That the Standing Committee be requested to print the existing Constitution, and the Constitution as proposed to be amended, and to distribute copies among the members of the Association.

(*Proceedings Eighth Meeting*, 1854.)

Resolved, That the Committee on the Constitution of the Association be continued, and requested to present for consideration at the next meeting a plan of By-Laws.

(*Proceedings Eighth Meeting*, 1854.)

MEMBERS

OF THE

AMERICAN ASSOCIATION

FOR THE

ADVANCEMENT OF SCIENCE.

NOTE. — Names of deceased members are marked with an asterisk (*); and those of members who, in 1840, formed the original "Association of American Geologists," are in small capitals. The figure at the end of each name refers to the meeting at which the election took place.

A.

Abbott, Gorham D., New York [7].
Abert, Col. J. J., Washington, D. C. [1].
*Adams, Prof. C. B., Amherst, Massachusetts [1].
Adamson, Rev. Wm., England [7].
Agassiz, Prof. Louis, Cambridge, Massachusetts [1].
Alexander, Dr. R. C., Bath, England [2].
Alexander, Prof. Stephen, Princeton, New Jersey [1].
Allen, Prof. E. A. H., New Bedford, Massachusetts [6].
Allen, George N., Oberlin, Ohio [5].
Allen, John H., Oxford, Maryland [6].
Allen, Zachariah, Esq., Providence [1].
Allston, R. F. W., Esq., Georgetown, South Carolina [3].
Allyn, Rev. Robert, E. Greenwich, Rhode Island [9].
*Ames, M. P., Esq., Springfield, Massachusetts [1].
Amory, Jonathan, Jamaica Plains, Massachusetts [8].
Andrews, Alonzo, Lewiston, Maine [7].

Andrews, E., M. D., Ann Arbor, Michigan [7].
Andrews, Prof. E. B., Marietta, Ohio [7].
Andrews, Prof. J. W., Marietta, Ohio [5].
Angell, Prof. James B., Providence [9].
Anthony, Charles H., Esq., Albany [6].
Anthony, Henry, Providence [9].
Anthony, J. G., Esq., Cincinnati, Ohio [1].
Appleton, Nathan, Esq., Boston [1].
Appleton, Thomas G., Boston [8].
Arden, Thomas B., Garrison's P. O., Putnam Co., New York [7].
Armsby, Prof. J. H., Albany [6].
Astrop, R. F., Crichton's Store, Burns Co., Virginia [7].
Austin, Samuel, Providence [9].
Ayres, William O., Esq., San Francisco, California [1].

B.

Bache, Prof. Alexander D., Washington, D. C. [1].
Bache, Dr. Franklin, Philadelphia [1].
Bacon, J. S., Pres., Washington, D. C. [1].
Bacon, Dr. John, Jr., Boston [1].
Bacon, William, Richmond, Berkshire Co., Massachusetts [7].
Baer, Prof. William, Sykesville, Maryland [8].
Bagg, Moses M., Utica, New York [4].
Bailey, Prof. J. W., West Point, New York [1].
Baird, Prof. S. F., Washington, D. C. [1].
Barlow, Thomas, Canastota, New York [7].
Barnard, F. A. P., Oxford, Mississippi [7].
Barnes, Capt. James, Springfield, Massachusetts [5].
Barratt, Dr. J. P., Barrattsville, South Carolina [3].
Bartlett, J. R., Providence [8].
Bartlett, Prof. W. H. C., West Point, New York [9].
Barton, Dr. E. H., New Orleans [9].
Bassnett, Thomas, Ottawa, Illinois [8].
Batchelder, J. M., Cambridge, Massachusetts [8].
Bean, Sidney A., Waukesha, Wisconsin [9].
Beck, Dr. C. F., Philadelphia [1].
*Beck, Prof. Lewis C., New Brunswick, New Jersey [1].
*Beck, Dr. T. Romeyn, Albany [1].

Bell, John G., New York [7].
Bell, Samuel N., Manchester, New Hampshire [7].
Berthoud, Edward L., Maysville, Mason Co., Kentucky [7].
Bigelow, Artemas, Newark, New Jersey [9].
Bingham, Rev. J. F., New York [7].
*Binney, Dr. Amos, Boston [1].
Binney, Amos, Esq., Boston [9].
*Binney, John, Esq., Boston [3].
Blake, William P., Esq., Washington, D. C. [2].
*Blanding, Dr. William, Rhode Island [1].
Blasius, Wilhelm, New York [7].
Blodget, Lorin, Washington, D. C. [7].
*Bomford, Col. George, Washington, D. C. [1].
Bond, George P., Esq., Cambridge, Massachusetts [2].
Bond, William C., Esq., Cambridge, Massachusetts [2].
Bonnycastle, Sir Charles, Montreal, Canada [1].
Borland, J. N., M. D., Boston [9].
Botta, Prof. Vincenzo, New York [9].
Bowditch, Henry J., M. D., Boston [2].
Boyden, Uriah A., Esq., Boston [2].
Boynton, John F., Esq., Syracuse, New York [4].
Bradford, Hezekiah, New York [7].
Brainerd, Prof. Jehu, Cleveland, Ohio [5].
Brant, James R., New York [9].
Britton, A. A., Keokuk, Iowa [7].
Brocklesby, Prof. John, Hartford, Connecticut [4].
Brooks, Rev. Charles, Boston [9].
Bross, William, Chicago, Illinois [7].
Brown, Andrew, Esq., Natchez, Mississippi [1].
Brown, John C., Esq., Providence [9].
Brown, Richard, Esq., Sydney, Cape Breton [1].
Brown, Prof. W. Leroy, Oakland, Mississippi [7].
Browne, Peter A., Esq., Philadelphia [1].
Buchanan, Robert, Esq., Cincinnati, Ohio [2].
Buckland, David, Brandon, Vermont [7].
Burgess, Rev. E., Ahmednuggur, India [1].
*Burnett, Waldo I., Esq., Boston [1].
Busher, James, Worcester, Massachusetts [9].

C.

Cabell, Prof. James L., University of Virginia [6].
Carey, H. C., Burlington, New Jersey [8].
Carley, S. T., Esq., Cincinnati, Ohio [5].
*Carpenter, Thornton, Camden, South Carolina [7].
*Carpenter, Dr. William M., New Orleans [1].
Carr, E. S., Albany [9].
Case, Hon. William, Cleveland, Ohio [6].
Cassels, Prof. J. Long, Cleveland, Ohio [7].
Caswell, Prof. Alexis, Providence [2].
Chandler, M. W. T., Philadelphia [8].
Channing, William F., Esq., Boston [2].
*Chapman, Dr. N., Philadelphia [1].
Chappellsmith, John, New Harmony, Indiana [7].
Chase, Rev. Benj., Natchez, Mississippi [7].
Chase, Prof. George I., Providence [1].
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Thompson, Dr. Alexander, Aurora, New York [6].
Thompson, Aaron R., New York [1].
Thompson, Dr. J. W., Wilmington, Delaware [9].
Thompson, John Edgar, Esq., Philadelphia [1].
Thompson, Robert, Columbus, Ohio [7].
*Thompson, Rev. Z., Burlington, Vermont [1].

Thurber, George, Esq., Providence [1].
Thurber, Isaac, Providence [9].
Thurston, E. M., Charleston, Maine [7].
Tobey, Dr. Samuel B., Providence [9].
Torrey, Dr. John, New York [1].
Torrey, Prof. Joseph, Burlington, Vermont [2].
Totten, Gen. J. G., U. S. A., Washington, D. C. [1].
Town, Salem, New Albany, Indiana [7].
*Townsend, John K., Esq., Philadelphia [1].
Townsend, Robert, Albany [9].
*Troost, Dr. Gerard, Nashville, Tennessee [1].
Trumbull, James H., Esq., Hartford, Connecticut [4].
Turner, Wm. C., Cleveland, Ohio [7].
Turner, Wm. W., Washington, D. C. [7].
Tuthill, Franklin, M. D., New York [8].
*Tyler, Rev. Edward R., New Haven, Connecticut [1].
Tyler, Moses, Detroit, Michigan [7].

V.

Vail, Prof. Hugh, Haverford, Pennsylvania [8].
Vancleve, John W., Dayton, Ohio [1].
Van Duzee, William S., M. D., Buffalo, New York [7].
Van Derpool, S. Oakley, Albany, New York [9].
Van Lennep, Rev. H., Constantinople, Turkey [1].
Van Pelt, Wm., M. D., Williamsville, Erie Co., New York [7].
*Vanuxem, Lardner, Esq., Bristol, Pennsylvania [1].
Van Vleck, J. M., Middletown, Connecticut [9].
Vaughan, Daniel, Esq., Cincinnati, Ohio [5].
Vaux, William S., Esq., Philadelphia [1].

W.

Wadsworth, James S., Esq., Genesee, New York [2].
Wagner, Tobias, Philadelphia [9].
Walker, Rev. Jas. B., Mansfield, Ohio [7].
Walker, Joseph (late U. S. A.), Platte City, Missouri [7].
*Walker, Sears C., Esq., Washington, D. C. [1].
*Walker, Hon. Timothy, Cincinnati, Ohio [4].
Walling, Henry F., Providence [9].

Warder, Dr. J. A., Cincinnati, Ohio [4].
Warren, Dr. John C., Boston [1].
Wayland, Dr. Francis P., Providence [9].
Webber, Dr. Samuel, Charlestown, New Hampshire [1].
*Webster, H. B., Esq., Albany, New York [1].
*Webster, Dr. J. W., Cambridge, Massachusetts [1].
*Webster, M. H., Esq., Albany, New York [1].
Webster, Nathan B., Portsmouth, Virginia [7].
Webster, William F., Providence [9].
Weed, Monroe, Esq., Wyoming, New York [6].
Wells, Dr. Thomas, New Haven, Connecticut [4].
Wentworth, Prof. Erastus, Carlisle, Pennsylvania [7].
Wescott, Rev. Isaac, New York [8].
West, Charles E., Esq., New York [1].
Wethrell, Prof. L., Lagrange, Kentucky [2].
Weyman, G. W., Esq., Pittsburg, Pennsylvania [6].
Wheatland, Dr. Henry, Salem, Massachusetts [1].
Whipple, W., Adrian, Michigan [7].
Whitney, Asa, Esq., Philadelphia [1].
Whitney, J. D., Esq., Northampton, Massachusetts [1].
Wilder, L., Esq., Hoosick Falls, New York [1].
Wilkes, Capt. Charles, U. S. N., Washington, D. C. [1].
Williams, Dr. Abraham V., New York [9].
Williams, Prof. Geo. P., Ann Arbor, Michigan [7].
Williams, Prof. L. D., Meadville, Pennsylvania [6].
Williams, Dr. P. O., Watertown, St. Lawrence Co., N. Y. [6].
Wills, Frank, New York [9].
Wilson, Prof. John, London, England [7].
Winlock, Prof. Joseph, Cambridge, Massachusetts [5].
Winslow, Rufus K., Esq., Cleveland, Ohio [6].
*Woodbury, Hon. L., Portsmouth, New Hampshire [1].
Woodhull, Lieut. Maxwell, U. S. A., Washington, D. C. [4].
Woodrow, Prof. James, Milledgeville, Georgia [7].
Worcester, Dr. Joseph E., Cambridge, Massachusetts [2].
Wormley, Theo. G., Columbus, Ohio [7].
Wright, Albert B., Perrysburg, Ohio [7].
*Wright, Dr. John, Troy, New York [1].
Wynne, Dr. James, Baltimore [8].

Wynne, Thomas H., Richmond, Virginia [8].

Y.

Yarnall, Prof. M., U. S. A. [8].
Youmans, E. L. Esq., Middlegrove, Saratoga Co., New York [6].
Young, Prof. Ira, Hanover, New Hampshire [7].

Addendum.

Bouve, Thomas T., Esq., Boston [1].
Hammond, Ogden, Esq., Charleston [3].

The above list contains six hundred and sixty-four names, of which fifty-nine are of deceased members.

MEMBERS ELECTED AT THE PROVIDENCE MEETING.*

Alexander, Prof. J. H., Baltimore.
Allyn, Rev. Robert, E. Greenwich, R. I.
Angell, Prof. James B., Providence.
Anthony, Henry, Providence.
Austin, Samuel, Providence.
Bartlett, Prof. W. H. C., West Point, N. Y.
Barton, Dr. E. H., New Orleans.
Bean, Sidney A., Waukesha, Wisc.
Bigelow, Artemas, Newark, N. J.
*Borland, J. N., M. D., Boston.
Botta, Prof. Vincenzo, New York.
Brant, James R., New York.
Brooks, Charles, Boston.
Brown, John C., Providence.
Busher, James, Worcester, Mass.
Carr, E. S., Albany.
Cherriman, J. B., Toronto, Canada.
Clark, Bishop T. M., Providence.
Cleaveland, Prof. C. H., Cincinnati.
Clement, H. H., Providence.
Clum, H. A., Le Roy, N. Y.
Collins, Dr. George L., Providence.
*Cooper, William, Hoboken, N. J.
Corliss, George H., Providence.
Dakin, Prof. F. E., Troy, N. Y.
Davis, Prof. N. K., Marion, Alabama.
De Wolf, Prof. John, Bristol, R. I.
Dyer, Elisha, Providence.
Ely, Dr. J. W. C., Providence.
*Farmer, Moses G., Boston.
Fisher, Dr. N. A., Norwich, Conn.
Fisher, Robert A., Providence.
Francford, Dr. E., Middletown, Conn.
Goddard, William, Providence.
Green, Dr. John W., New York.
Greene, Samuel, Woonsocket, R. I.
Greene, Prof. Samuel S., Providence.
Greene, Thos. A., New Bedford, Mass.
Grosvenor, Dr. William, Providence.
Haines, William S., Providence.
Haven, Samuel F., Worcester, Mass.
Hazard, Rowland, Peace Dale, R. I.
Holme, W. H., Providence.
Holland, Joseph B., Monson, Mass.
Hollowell, W. E., Alabama.
Holton, J. F., New York.
Hunt, George, Providence.
Ives, Moses B., Providence.
Jones, Rev. George, U. S. N.
Loomis, L. Charles, Wilmington, Del.
Lockwood, Moses B., Providence.
Mahan, Prof. D. H., West Point, N. Y.
*Mason, Owen, Providence.
Mason, R. C., Appleton, Wisc.
Nason, Henry B., Worcester, Mass.

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* This list also includes those who signed the Constitution, and paid the assessment, without being formally elected. Those marked with an asterisk paid the assessment, without signing the Constitution or being formally elected.

Okie, Abraham H., Providence.
Olney, Stephen T., Providence.
Ordway, John M., Roxbury, Mass.
Parsons, Dr. Charles H., Providence.
Parsons, Dr. Usher, Providence.
Peck, Lieut. G. W., U. S. T. E.
Peters, Dr. C. H. F., Cambridge, Mass.
Plumb, Dr. Ovid, Salisbury, Conn.
Ray, Dr. Isaac, Providence.
Richards, Rev. W. C., Providence.
Rodman, William M., Providence.
Ruggles, Samuel B., New York.
Savill, Henry M., Boston.
Shaw, Edward, Washington, D. C.
Shepard, Thomas P., Providence.
Smith, Prof. Francis H., Charlottesville, Va.
Smith, James Y., Providence.
Smith, Dr. Lyndon A., Newark, N. J.
Smith, Sanderson, New York.
Snow, E. M., Providence.
Stevens, Prof. M. C., Richmond, Ind.
Stone, Rev. Edwin M., Providence.
Suckley, Dr. George, U. S. A.
Tefft, Thomas A., Providence.
Thayer, Dr. S. W., Burlington, Vt.
Thompson, Dr. J. W., Wilmington, Del.
Thurber, Isaac, Providence.
Tobey, Dr. Samuel B., Providence.
Townsend, Robert, Albany.
Van Derpool, S. Oakley, Albany.
Van Vleck, J. M., Middletown, Conn.
Vose, George B., New York.
Wagner, Tobias, Philadelphia.
Walling, Henry F., Providence.
Wayland, Francis P., Providence.
Webster, William F., Providence.
Williams, Dr. Abraham V., New York.
Wills, Frank, New York.
Wilson, George F., Providence.
Wright, Chauncey, Cambridge, Mass.

ADDRESS

OF

PROFESSOR JAMES D. DANA,

AND

REPORT

ON THE

RECENT PROGRESS OF ORGANIC CHEMISTRY,

BY

DR. WOLCOTT GIBBS.

ADDRESS

OF

PROFESSOR JAMES D. DANA,

PRESIDENT OF THE AMERICAN ASSOCIATION FOR THE YEAR 1854,

ON RETIRING FROM THE DUTIES OF PRESIDENT.

MR. PRESIDENT AND GENTLEMEN OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE: —

IT is a noble object that invites us to these annual gatherings. Leaving the broils of the world to others, we come to contemplate together the teachings of God in nature. We come with faith in that word which is written around and within us, believing in the truthfulness of the revelation, and knowing that he who approaches it with an inquiring, teachable spirit, ever wakeful to the still, small voice, and forgetful of ambitious self, shall find the truth and feel its benign influence. We aim to decipher some new words in the volume of Nature, that we may learn the will of Him who has ordered all things well, and comprehend more fully His laws in the government of the universe.

In the use of this word *law*, as applied to nature, we are often grossly misunderstood. Says a recent writer, somewhat contemptuously, "The philosopher knows no better the cause of the law of gravitation than the ignorant man." The author, in his simplicity, is unaware that laws, not causes, are the end of true philosophy. We seek to study out the method of God's doings in nature, and enunciations of this

his method or will are what is meant by the "laws of nature." If those who look coldly on science knew better its aims, we should hear less of the infidelity of the term *law*, and find fewer infidels or rejecters of that revelation which God has spread out before us.

We know that this is not the only revelation; that another tells man of his duties and responsibilities, of the celestial sympathy which surrounds him, and his immortal destiny,— subjects far beyond the teachings of physical or brute nature. The one is but the complement of the other; the two harmonious in their truths, as in their exalted origin.

Although different in scope, they teach alike humility and reverence. To the vision of science, there is nothing in nature minute or unworthy. It pities the feeling that would turn with contempt from the meanest created thing. For it stops not with mere externals, but distinguishes profound relations and universal laws beneath the surface. Exhibit the leaf of a plant, or an animalcule, under a microscope, to a person of good taste, as the phrase is, and he will appreciate the beautiful coloring and the wonderful forms displayed; for form and color are media of real beauty in nature, and there are eyes enough to admire. But let another look whose mind has been deeply imbued with science, and profounder beauties open to his view; the object is not an isolated thing of exquisite tints or admirable shape; it takes its place in the vast system of nature, and derives grandeur beyond estimate from its relations to that system. Nature becomes a living expression — as full as is possible in finite language — of the perfections of the Supreme Architect, with whom to create has ever been to evolve beauty amid displays of wisdom and beneficence. And the devout mind finds deeper meaning in the words Reverence and Humility, and ceaseless promptings to his Gratitude and Love.

We have reason for gratulation that correct views respecting Natural and Physical Science are rapidly becoming general throughout our land. Among its votaries, while every new fact is heartily welcomed to its place, there is a strong distaste for human fancies as a substitute for divine truths, or for theories without a broad basis of ascertained facts. Moreover, there is, at large, an appreciation of the value of science, not merely for its baser purpose of turning everything into gold, but for its nobler end of opening the earlier revelation. The means also for the prosecution of science are gradually extending, and it is favorably recognized, with hardly an exception, in all the literary institutions of the country, though not generally raised to that honorable place which it may expect in the future.

Happily, too, there is much to encourage extended research in the opportunities now before us for the publication of elaborate original memoirs. Not, indeed, in the small treasury of this Association, which boasts more of the mutual good-will and the ideas it elicits than of its moneyed resources, but in the transactions of different scientific societies or academies and the periodical press, and pre-eminently in the Smithsonian Institution, through the munificence of Smithson, who gave his fortune expressly for this end, — the *increase* and *diffusion* of knowledge among men. It has been recently attempted to strike out the *idea*, if not the *word increase*, and, in equal wisdom, it was thought to *diffuse knowledge* over this extended territory and the world by stationing a collection of books at Washington. Fortunately for knowledge and the country such counsels have not prevailed. The funds for publication and for its other legitimate purposes are, in fact, lamentably small. An annual income of thirty-one thousand dollars, diminished one third by the incidental expenses of the Institution, is a meagre allowance. The complete elaboration and publication yearly of the meteorological results, from the facts now constantly coming in, would alone require half the available publication fund; and, on account of its expense, has not yet been attempted. And what shall be said of other departments, and the costly illustrations which detailed investigations require? It is well the country has what it has. It would be vastly more for its interests, and for the increase and diffusion of knowledge among men, if the resources for publication were tenfold larger.

In selecting a topic for this occasion, I have not been without perplexity. Before an Association for the Advancement of Science, — science in its wide range, — a discourse on the progress of science in America for the past year would seem legitimate. Yet it is a fact that the original memoirs in most departments, published within that period, would make a very meagre list. Moreover, it is too much to expect of any one to roam over other's territories, lest he ignorantly gather for you noxious weeds. I have, therefore, chosen to confine myself to a single topic, that of Geology; and I propose, instead of simply reviewing recent geological papers, to restrict myself to some of the general conclusions that flow from the researches of American geologists, and the bearing of the facts or conclusions on geological science. I shall touch briefly on the several topics, as it is a subject that would more easily be brought into the compass of six hours than one. In drawing conclusions among conflicting opinions, or on points where no opinion has been expressed, I shall endeavor to treat the subject and the views of others in all fairness, and shall be satisfied if those who differ from me shall acknowledge that I have honestly sought the truth.

In the first place, we should have a clear apprehension of the intent or aim of Geological Science. It has been often said, that Geology is a *history*, the records of which are written in the rocks; and such is its highest department. But is this clearly appreciated? If so, why do we find text-books, even the one highest in authority in the English language, written back end foremost, — like a History of England commencing with the reign of Victoria. In history, the phases of every age are deeply rooted in the preceding, and intimately dependent on the whole past. There is a literal unfolding of events as time moves on, and this is eminently true of Geology.

Geology is not simply the science of rocks, for rocks are but incidents in the earth's history, and may or may not have been the same in distant places. It has its more exalted end, — even the study of the progress of life from its earliest dawn to the appearance of man; and instead of saying that fossils are of use to determine rocks, we should rather say that the rocks are of use for the display of the succession of fossils. Both statements are correct; but the latter is the fundamental truth in the science.

From the progress of life, geological time derives its division into Ages, as has been so beautifully exhibited by Agassiz. The successive phases in the progress of life are the great steps in the earth's history. What if in one country the rocks make a consecutive series without any marked interruption between two of these great ages, while there is a break or convenient starting-point in another; does this alter the actuality of the ages? It is only like a book without chapters in one case, and with arbitrary sections in another. Again, what if the events characteristic of an age — that is, in Geology, the races of plants or animals — appear to some extent in the preceding and following ages, so that they thus blend with one another? It is but an illustration of the principle just stated, that *time is one.* Ages have their progressive development, flowing partly out of earlier time, and casting their lights and shadows into the far future. We distinguish the ages by the culmination of their great characteristics, as we would mark a wave by its crest.

Divisions of time *subordinate* to the great ages will necessarily depend on revolutions in the earth's surface, marked by abrupt transitions either in the organic remains of the region, or in the succession of rocks. Such divisions are not universal. Each continent has its own periods and epochs, and the geologists of New York and other States have wisely recognized this fact, disregarding European *stages* or subdivisions. This is as true a principle for the Cretaceous and Tertiary, as for the Silurian and Devonian. The usurpation of Cromwell made an epoch in English annals; not in the French or Chinese. We should study most carefully the records, before admitting that any physical event in America was contemporaneous with a similar one in Europe. The unity in geological history is in the progess of life and in the great physical causes of change, not in the succession of rocks.

The geological ages, as laid down by Agassiz, are the following:— I. The Age of Fishes, including the Silurian and Devonian; II. The Age of Reptiles, embracing from the Carboniferous through the Cretaceous; III. The Age of Mammals, the Tertiary and Post-tertiary; IV. The Age of Man, or the recent era;—*fishes* being regarded as the highest and characteristic race of the first age; *reptiles* of the second; and *mammals* of the third.

More recent researches abroad, and also the investigations of Mr. Hall in this country, have shown that the supposed Fish remains of the Silurian are probably fragments of Crustacea, if we except those of certain beds near the top of the Silurian; and hence the *Age of Fishes* properly begins with the Devonian. What then is the Silurian? It is pre-eminently the Age of Mollusks.

Unlike the other two Invertebrate sub-kingdoms, the *Radiate* and *Articulate*, which also appear in the earliest fossiliferous beds, the *Molluscan* sub-kingdom is brought out in all its grander divisions. There is not simply the type, but the type analyzed or unfolded into its several departments, from the Brachiopods and Bryozoa up to the highest group of all, the Cephalopods. And among these Cephalopods, although they may have been inferior in grade to some of later periods, there were species of gigantic size, the shell reaching a length of ten or twelve feet. The Silurian is therefore most appropriately styled the *Molluscan Age*.

The Palæozoic Trilobites were the lowest among Crustacea, and Crustacea rank low among Articulates. Moreover, Crustacea (and the Articulata in general) did not reach their fullest development until the Human Era.

The Radiata were well represented in the Silurian periods; but, while inferior to the Mollusca as a sub-kingdom, only corals and crinoids, the lower fixed or vegetative species, with rare exceptions, occur in the Silurian or Molluscan Age.

The Articulata and Radiata thus begin early, but with only the lower forms in each, and neither is a leading class in any age.

Viewing the history, then, *zoölogically*, the ages are — the Age of Mollusks, of Fishes, of Reptiles, of Mammals, of Man.

We may now change the point of view to the Vegetable Kingdom. The ages thence indicated would be three: —

I. The *Age of Algæ*, or marine plants, corresponding to the Silurian and Devonian.

II. The *Age of Acrogens*, or flowerless trees, that is, the Lepidodendra, Sigillariæ, and Calamites, — corresponding to the Coal Period and Permian; — a name first proposed by Brongniart, and which may still be retained, as it is far from certain that the Sigillariæ and Calamites are most nearly related to the Coniferæ.

III. The *Age of Angiosperms*, or our common trees, like the Oak, Elm, &c., beginning with the Tertiary.

The interval between the second and third of these ages is occupied mainly by Coniferæ, the Pine tribe, and Cycadeæ, the true Gymnosperms, species of which were abundant in the Coal Period, and have continued common ever since. The Coniferæ, in the simplicity of their flowers and their naked seeds, are next akin to the Acrogens or flowerless trees. Although in the main a flowerless vegetation, for the few supposed remains of flowers observed abroad have been recently referred to undeveloped leaf-buds, it appears probable from the observations of Dr. Newberry, that there were some true flowers over the Ohio prairies, — apparently monocotyledonous, and related to the Lily tribe. But no traces of Palms or monocotyledonous trees have been found in the coal-fields of this country.

Combining the results from the animal and vegetable kingdoms, we should introduce the Age of Acrogens, for the Coal Period and Permian, between the Age of Fishes and Age of Reptiles, — a space in time zoölogically occupied by the overlapping of these two ages.

The order then reads, the Age of MOLLUSKS, of FISHES, of ACROGENS or coal plants, of REPTILES, of MAMMALS, of MAN.

The limits of these ages are as distinct as history admits of; their blendings where they join, and the incipient appearance of a type before the age it afterwards characterizes fully opens, are in accordance with principles already explained.

The reality of progress from lower to higher forms is not more strongly marked in these names, properly applied, than in the rocks. If, hereafter, mammals, reptiles, or fishes are found a little lower than now known, it will be changing but a sentence in the history, — not the grand idea which pervades it.

A theory lately broached by one whose recent death has caused universal grief to science, supposes that the Reptilian was an age of diminished life, between the two extremes in time, the Palæozoic and Mammalian Ages. But, in fact, two grand divisions of animals, the Molluscan and Reptilian, at this time reach their climax and begin their decline, and this is the earliest instance of the highest culmination of a grand zoölogical type.

Preceding the Silurian or Molluscan Age, there is the AZOIC AGE, or *age without animal life.* It was so named by Murchison and De Verneuil, and first recognized in its full importance, and formally announced, in this country, in the Geological Report of Messrs. Foster and Whitney, although previously admitted, in an indefinite way, by most geologists.

It embraces all the lowest rocks, up to the Silurian, for much of the lowest granite cannot be excluded.

The actual absence of animal life in the so-called Azoic Age in this country is rendered highly probable, as Foster and Whitney show, by the fact that many of the rocks are slates and sandstones, like fossiliferous Silurian rocks, and yet have no fossils; and moreover, the beds on this continent were uplifted and folded, and, to a great extent, crystallized, on a vast scale, before the first Silurian layers were deposited. A grand revolution is here indicated, apparently the closing event of the early physical history of the globe.

As plants may live in water too hot or impure for animals, and moreover, since all nature exemplifies the principle that the earth's surface was occupied with life as soon as fitted, and with the highest forms the conditions of the time allowed, we may reasonably infer that there may have been in Azoic times marine species and plant-infusoria, forms adapted to aid in the earth's physical history; and thus vegetation may have long preceded animal life on the globe.

After these general remarks on the divisions of Geological time, I now propose to take up the characteristic features and succession of events in American Geology.

In the outset we are struck with the comparative simplicity of the North American continent, both in form and structure. In *outline* it is a triangle, the simplest of mathematical figures; in *surface*, it is only a vast plain lying between two mountain ranges, one on either border, the Appalachian from Labrador to Alabama on the east, the Rocky Mountains on the west; and on its *contour* it has water, east, west, north, and south.

Observe too that its border heights are proportioned to the size of the oceans. A *lofty* chain borders the Pacific, a *low* one the narrow Atlantic, while the small Arctic is faced by no proper mountain range.

This principle, that the highest mountains of the continents face the largest oceans, is of wide application, and unlocks many mysteries in Physical Geography. South America lies between the same oceans as North America; it has its eastern low range, its western Andes; and as the oceans widen southward, the continent is there pinched up almost to a narrow mountain ridge. It differs from North America in having a large expanse of ocean, the Atlantic, on the north, and, correspondingly, it has its northern mountain ridge. The world is full of such illustrations, but I pass them by.

This simplicity of ocean boundary, of surface features, and of outline, accounts for the simplicity of geological structure in North America; or we may make the wider statement, that all these qualities are some way connected with the positions and extent of the oceans, they seeming to point to the principle, that the subsidence of the oceanic basins had determined the continental features; and that both results were involved in the earth's gradual refrigeration, and consequent contraction.

America has thus the simplicity of a single evolved result. Europe, on the contrary, is a world of complexities. It is but one corner of the Oriental continent, which includes Europe, Asia, and Africa, and while the ocean bounds it on the north and west, continental lands inclose it on the south and east. It has ever been full of cross purposes. American strata often stretch from the Atlantic west beyond the Mississippi; and east of the Rocky Mountains, it has but one proper mountain range of later date than the Silurian. Europe is much broken up into basins, and has mountains of all ages: even the Alps and Pyrenees are as recent as the Tertiary.

This wide contrast accounts for the greater completeness or generality of American revolutions, the more abrupt limits of periods, and clearer exhibition of many geological principles.

The geological structure of this country has been made known through the combined researches of a large number of investigators. The names of MACLURE, SILLIMAN, EATON, lead off the roll; HITCHCOCK, the Professors ROGERS, the well-known GEOLOGISTS OF THE NEW YORK SURVEY, OWEN, PERCIVAL, MORTON, CONRAD, TUOMEY, and many others, have made large contributions to the accumulating results. Yet the *system* may be said to have been mainly laid open by four sets of observers, — MORTON for the Cretaceous; CONRAD for the Tertiary; the NEW YORK GEOLOGISTS for the Palæozoic strata; and the Professors ROGERS for the Carboniferous beds and the Appalachians.

The succession of Silurian and Devonian rocks in the State of New York is the most complete in the country, and it was well for the science that its rocks were so early studied, and with such exactness of detail. The final display of the Palæontology by Mr. James Hall has given great precision to the facts, and the system has thereby become a standard of comparison for the whole country, and even for the world.

This accomplished, the carboniferous rocks were still to be registered, and the grand problem of New England Geology solved. The Professors Rogers, in the surveys of Pennsylvania and Virginia, followed out the succession of strata from the Devonian through the Coal Period, and thus, in a general way, completed the series. And more than this, they unravelled with consummate skill the contortions among the Appalachians, bringing order out of confusion, and elucidating a principle of mountain-making which is almost universal in its application. They showed that the Silurian, Devonian, and Carboniferous strata, which were originally laid out in horizontal layers, were afterwards pressed on to the northwestward, and folded up till the folds were of mountain height, and that thus the Appalachians had their origin; and also that, by the escaping heat of those times of revolution, extensive strata were altered, or even crystallized.

This key soon opened to us a knowledge of New England Geology, mainly through the labors of Mr. Hall, and also of Professor H. D. Rogers, following up the survey of President Hitchcock; and now these so-called primary rocks, granite, gneiss, schists, and crystalline limestones, once regarded as the oldest crystallizations of a cooling globe, are confidently set down as for the most part no older than the Silurian, Devonian, and Carboniferous of New York and Pennsylvania.

Let us now briefly review the succession of epochs in American geological history.

The Azoic Age ended, as was observed, in a period of extensive metamorphic action and disturbance, — in other words, in a vast revolution. At its close, some parts of the continent were left as dry land, which appear to have remained so, as a general thing, in after times; for no subsequent strata cover them. Such are a region in Northern New York, others about and beyond Lake Superior, and a large territory across the continent from Labrador westward, as recognized by Messrs. Whitney and Hall, and the geologists of Canada.

The Silurian or Molluscan Age next opens. The lowest rock is a sandstone, one of the most widely spread rocks of the continent, stretching from New England and Canada south and west, and reaching beyond the Mississippi, — how far is not known. And this first leaf in the record of life is like a title-page to the whole volume, long afterwards completed; for the nature of the history is here declared in a few comprehensive enunciations.

1. The rock, from its thin, even layers, and very great extent, shows the wide action of the ocean in distributing and working over the sands of which it was made; and the ocean ever afterwards was the most active agency in rock-making.

2. Moreover, ripple-marks, such as are made on our present sea-shores or in shallow waters, abound in the rock, both through the east and west, and there are other evidences also of moderate depths, and of emerged land; they all announce the wonderful fact, that even then, in that early day, when life first began to light up the globe, the continent had its existence: — not in embryo, but of full-grown extent; and the whole future record is but a working upon the same basis, and essentially within the same limits. It is true that but little of it was above the sea, but equally true that little of it was at great depths in the ocean.

3. Again, in the remains of life which appear in the earliest layers of this primal rock, three of the four great branches of the Animal Kingdom are represented, — Mollusks, Trilobites among Articulates, and Corals and Crinoids among Radi-

ates, — a sufficient representation of life for a title-page. The New York beds of this rock had afforded only a few Mollusks; but the investigations of Owen in Wisconsin have added the other tribes; and this diversity of forms is confirmed by Barrande in his Bohemian researches.

Among the genera, while the most of them were ancient forms that afterwards became extinct, and through succeeding ages thousands of other genera appeared and disappeared, the very earliest and most universal was one that now exists, — the genus *Lingula*, — thus connecting the extremes of time, and declaring most impressively the unity of creation. Mr. T. S. Hunt, of the Canada Geological Survey, recently discovered that the ancient shell had the anomalous chemical constitution of bones, being mainly phosphate of lime; and afterwards he found in a modern Lingula the very same composition, — a further announcement of the harmony between the earliest and latest events in geological history.

This earliest sandstone, — called in New York the Potsdam sandstone, — and the associate Calciferous Sand-rock, mark off the *First Period* of the Molluscan Age, — the POTSDAM PERIOD, as it may be called.

Next followed the TRENTON PERIOD, — a period of limestones, (the Trenton limestone among them,) equal to the earlier beds in geographical limits, and far more abundant in life, for some beds are literally shells and corals packed down in bulk: yet the species were new to the period, the former life having passed away; and even before the Trenton Period closed, there were one or two epochs of destruction of life followed by new creations. The formation of these limestone beds indicates an increase in the depth of the continental seas, — an instance of the oscillation of level to which the earth's crust was almost unceasingly subject through all geological ages until the present.

After the Trenton Period, another change came over the continent, and clayey rocks or shales were formed in thick deposits in New York, and south, — the Utica slate and Hudson River shales,—while limestones were continued in the West. This is the HUDSON PERIOD; and with it, the *Lower Silurian* closed.

The seas were then swept of their life again, and an abrupt transition took place both in species and rocks. A conglomerate covered a large part of New York and the States south, its coarse material evidence of an epoch of violence and catastrophe: and with this deposit the *Upper Silurian* began.

The Upper Silurian had also its three great periods,—the NIAGARA, the ONONDAGA, and the LOWER HELDERBERG, besides many subordinate epochs,—each characterized by its peculiar organic remains,—each evidence of the nearly or quite universal devastation that preceded it, and of the act of omnipotence that reinstated life on the globe,—each, too, bearing evidence of shallow or only moderately deep waters when they were formed; and the Onondaga Period—the period of the New York salt rocks—telling of a half-emerged continent of considerable extent.

Another devastation took place, and then opened, as De Verneuil has shown, the Devonian Age, or *Age of Fishes*. It commenced, like the Upper Silurian, with coarse sandstones, evidence of a time of violence; these were followed by another grit rock, whose few organic remains show that life had already reappeared. Then another change,—a change evidently in depth of water,—and limestones were forming over the continent, from the Hudson far westward: the whole surface became an exuberant coral reef, far exceeding in extent, if not in brilliancy, any modern coral sea; for such was a portion, at least, of the UPPER HELDERBERG Period.

Again there was a general devastation, leaving not a trace of the former life in the wide seas; and where were coral reefs, especially in the more eastern portion of the continental seas, sandstones and shales accumulated for thousands of feet in thickness, with rarely a thin layer of limestone. Thus passed the HAMILTON, CHEMUNG, and CATSKILL Periods, of the Devonian age. The life of these regions, which in some epochs was exceedingly profuse, was three or four times destroyed and renewed: — not renewed by a re-creation of the same species, but by others; and although mostly like the earlier in genera, yet each having characteristic marks of the period to which it belonged. And while these Devonian Periods were passing, the first land plants appeared, foretellers of the Age of Verdure, next to follow.

Then come vast beds of conglomerate, a natural opening of a new chapter in the record; and here it is convenient to place the beginning of the Carboniferous Age, or the Age of ACROGENS. Sandstones and shales succeeded, reaching a thickness, in Pennsylvania and New Jersey, according to Professor Rogers, of thousands of feet; while in the basin of the Ohio and Mississippi, in the course of this era, the Carboniferous limestone was forming from immense crinoidal plantations in the seas.

Another extermination took place of all the beautiful life of the waters, and a conglomerate or sandstone was spread over the encrinital bed: and this introduced the true coal period of the Carboniferous Age; — for it ended in leaving the continent, which had been in long-continued oscillations, quite emerged. Over the regions where encrinites were blooming, stretch out vast prairies or wet meadows of the luxuriant coal vegetation. The old system of oscillations of the surface still continues, and many times the continent sinks to rise again, — in the sinking, extinguishing all continental life, and exposing the surface to new depositions of sandstone, clays, or limestone over the accumulated vegetable remains; in the rise, depopulating the seas by drying them up, and preparing the soil for verdure again, or at times convulsive movements of the crust carry the seas over the land, leaving destruction behind: and thus by repeated alternations the coal period passes, some six thousand feet of rock and coal-beds being formed in Pennsylvania, and fourteen thousand feet in Nova Scotia.

I have passed on in rapid review, in order to draw attention to the series or succession of changes, instead of details. So brief an outline may lead a mind not familiar with the subject to regard the elapsed time as short; whereas to one who follows out the various alternations and the whole order of events, the idea of *time immeasurable* becomes almost oppressive.

Before continuing the review, I will mention some conclusions which are here suggested.

I. In the first place, through the periods of the Silurian and Devonian, at twelve distinct epochs, at least, the seas over this American continent were swept of all, or nearly all, existing life, and as many times it was repeopled: and this is independent of many partial exterminations and renewals of life that at other times occurred.

If Omnipotent Power had been limited to making *monads* for after development into higher forms, many a time would the whole process have been utterly frustrated by hot water, or by mere changes of level in the earth's crust, and creation would have been at the mercy of dead forces. The surface would have required again and again the sowing of monads, and there would have been a total failure of crops after all; for these exterminations continue to occur through all geological time into the Mammalian Age.

II. Again: I have observed that the continent of North America has never been the deep ocean's bed, but a region of comparatively shallow seas, and at times emerging land; and was marked out in its great outlines even in the earliest Silurian. The same view is urged by De Verneuil, and appears now to be the prevailing opinion among American geologists. The depth at times may have been measured by the thousand feet, but not by miles.

III. During the first half of the lower Silurian era, the whole east and west were alike in being covered with the sea. In the first or Potsdam Period, the continent was just beneath its surface. In the next or Trenton Period, the depth was greater, giving purer waters for abundant marine life. Afterwards, the East and West were in general widely diverse in their formations; limestones, as Mr. Hall and the Professors Rogers have remarked, were in progress over the West, that is, the region, now the great Mississippi Valley, beyond the Appalachians, while sandstones and shales were forming, from Northeastern New York, south and southwest through Virginia. The former, therefore, has been regarded as an area of deeper waters, the latter as, in general, shallow, when not actually emerged. In fact, the region towards the Atlantic border, afterwards raised into the Appalachians, was already, even before the Lower Silurian era closed, the higher part of the land: it lay as a great reef or sand-bank, partly hemming in a vast continental lagoon where corals, encrinites, and mollusks grew in profusion, thus separating more or less perfectly the already existing Atlantic from the interior waters.

IV. The oscillations or changes of level over the continent, through the Upper Silurian and Devonian, had some reference to this border region of the continent: the formations approach or recede from it, and sometimes pass it, according to the limits of the oscillation eastward or westward. Along the course of the border itself there were deep subsidences in slow progress, as is shown by the thickness of the beds. It would require much detail to illustrate these points, and I leave them with this bare mention.

The Hudson River and Champlain valleys appear to have had their incipient origin at the epoch that closed the Lower Silurian; for while the preceding formations cross this region and continue over New England, the rocks of the Niagara and Onondaga Periods (the first two of the Upper Silurian) thin out in New York before reaching the Hudson River. Mr. Logan has recognized the division of America to the northeast into two basins by an anticlinal axis along Lake Champlain, and observes also that the disturbances began as early, at least, as the close of the Lower Silurian, mentioning, too, that there is actually a want of conformity at Gaspé between the beds of the Upper and Lower Silurian,—

another proof of the violence that closed the Lower Silurian era.

But let us pass onward in our geological record.

All the various oscillations that were in slow movement through the Silurian, Devonian, and Carboniferous Ages, and which were increasing their frequency throughout the last, raising and dipping the land in many alternations, were premonitions of the great period of revolution, — so well elucidated, as already observed, by the Professors Rogers, — when the Atlantic border, from Labrador to Alabama, long in preparation, was at last folded up into mountains, and the Silurian, Devonian, and Carboniferous rocks were baked or crystallized. No such event had happened since the revolution closing the Azoic Period. From that time on, all the various beds of succeeding ages up to the top of the Carboniferous had been laid down in horizontal or nearly horizontal layers, over New England as well as in the West, — for the continent from New England westward, we have reason to believe, was then nearly a plain, either above or below the water; there had been no disturbances except some minor uplifts: the deposits, with small exceptions, were a single unbroken record, until this Appalachian revolution.

This epoch, although a time of vast disturbances, is more correctly contemplated as an epoch of the slow measured movement of an agency of inconceivable power, pressing forward from the ocean towards the northwest; for the rocks were folded up without the chaotic destruction that sudden violence would have been likely to produce. Its greatest force and its earliest beginning was to the northeast. I have alluded to the disturbance between the Upper and Lower Silurian beds of Gaspé, to the north: another epoch of disturbance, still more marked, preceded, according to Mr. Logan, the Carboniferous beds in those northeastern regions; and New England, while a witness to the profound character and thoroughness of the Appalachian revolution, attests also to the greater disturbance towards its northern limits. Some of the Carboniferous strata were laid down here in Rhode Island, as clay and sand and layers of vegetable *débris:* they came forth from the Appalachian fires as you now have them, the beds contorted, the coal layers, a hard siliceous anthracite or even graphite in places, the argillaceous sands and clays, crystallized as talcose schist, or perhaps gneiss or syenite.

These very coal-beds, so involved in the crystalline rocks, are part of the proof that the crystallization of New England took place after the Coal Era. Fossils in Maine and Massachusetts add to the evidence; the quiet required by the continent for the regular succession and undisturbed condition of the rocks of the Silurian, Devonian, and Carboniferous formations, shows that in neither of these ages could such vast results of metamorphic action and upheaval have taken place.

The length of time occupied by this revolution is beyond all estimate. Every vestige of the ancient Carboniferous life of the continent disappeared before it. In Europe, a Permian Period passed, with its varied life; yet America, if we may trust negative evidence, still remained desolate. The Triassic Period next had its profusion of living beings in Europe, and over two thousand feet of rock; America through all, or till its later portions, was still a blank: not till near the beginning of the Jurassic Period do we find any traces of new life, or even of another rock above the Carboniferous.

What better evidence could we have than the history of the oscillations of the surface, from the earliest Silurian to the close of the Carboniferous Age, and the final cresting of the series in this Appalachian revolution, that the great features of the continent had been marked out from the earliest time? Even in the Azoic, the same northeast and southwest trend may be observed in Northern New York and beyond Lake Superior, showing that, although the course of the great Azoic lands was partly east and west, the same system of dynamics that characterized succeeding ages was then to some extent apparent.

The first event in the records after the Appalachian revolution, was the gathering up of the sands and rolled fragments of the crystallized rocks and schists along the Atlantic border into beds; not over the whole surface, but in certain valleys, which lie parallel with the Appalachian chain, and which were evidently a result of the foldings of that revolution. The beds are the red sandstones and shales, which stretch on for one hundred and twenty miles in the Connecticut valley; and similar strata occur in Southeastern New York, in New Jersey, Virginia, and North Carolina. These long valleys are believed to have been estuaries, or else river courses.

The period of these deposits is regarded as the earlier Jurassic by Professor W. B. Rogers. Dr. Hitchcock supposes that a portion of the preceding or Triassic Period may be represented.

Many of the layers show, by their shrinkage cracks, ripple-marks, and footprints, as others have observed, that they were formed in shallow waters, or existed as exposed mud-flats. But they accumulated till they were over a thousand feet thick in Virginia, and in New England two or three thousand, according to the lowest estimate. Hence the land must have been sinking to a depth equal to this thickness, as the accumulation went on, since the layers were formed successively at or near the surface.

Is it not plain, then, that the oscillations, so active in the Appalachian revolution, and actually constituting it, had not altogether ceased their movements, although the times were so quiet that numerous birds and reptiles were tenants of the Connecticut region? Is it not clear that these old valleys, occurring at intervals from Nova Scotia to South Carolina, originally made by foldings of the earth's crust, were still sinking?

And did not the tension below of the bending rocks finally cause ruptures? Even so: and the molten rock of the earth's interior which then escaped, through the crystalline rocks beneath and the overlying sandstone, constitutes the trap mountains, ridges, and dikes, thickly studding the Connecticut Valley, standing in palisades along the Hudson, and diversifying the features of New Jersey and parts of Virginia and North Carolina. The trap is a singularly constant attendant on the sandstone, and everywhere bears evidence of having been thrown out soon after the deposition of the sandstone, or in connection with the formation of its later beds. Even the small sandstone region of Southbury, Ct., has its trap. Like the Appalachian revolution, this epoch had its greatest disturbances at the north.

Thus ended in fire and violence, and probably in submergence beneath the sea, the quiet of the Connecticut valley, where lived, as we now believe, the first birds of creation; kinds that were nameless, until, some countless ages afterwards, President Hitchcock tracked them out, found evidence that they were no unworthy representatives of the feathered tribe, and gave them and their reptile associates befitting appellations.

Such vast regions of eruptions could not have been without effusions of hot water and steam, and copious hot springs. And may not these heated waters and vapors, rising through the crystalline rocks below, have brought up the copper ores, that are now distributed, in some places, through the sandstone? The same cause, too, may have given the prevalent red color to the rock, and produced changes in the adjoining granite.

After the era of these rocks, there is no other American record during the European Jurassic Period.

In the next or Cretaceous Period, the seas once more abound in animal life. The position of the cretaceous beds around the Atlantic border show that the continent then stood above the sea very much as now, except at a lower level. The Mississippi valley, which, from the Silurian, had generally been the region of deeper waters, was even in cretaceous times occupied to a considerable extent by the sea,— the Mexican Gulf then reaching far north, even high up the

Missouri, and covering also a considerable part of Texas and the Rocky Mountain slope.

An age later, the cretaceous species had disappeared, and the Mammalian Age (or the Tertiary, its first Period) begins, with a wholly new Fauna, excepting, according to Professor Tuomey, some half a dozen species, about which however there is much doubt. The continent was now more elevated than in the preceding age, and the salt waters of the Mexican Gulf were withdrawn from the region of Iowa and Wisconsin, so as not to reach beyond the limits of Tennessee.

Two or three times in the course of the Tertiary Period, the life of the seas was exterminated, so that the fossils of the later Tertiary are not identical with any in the earliest beds, — excluding some fish remains, species not confined to the coast waters. The crust of the earth was still oscillating; for the close of the first Tertiary epoch was a time of subsidence; but the oscillation or change of level was slight, and by the end of the Tertiary, the continent on the east stood within a few feet of its present elevation, while the Gulf of Mexico was reduced nearly to its present limits.

I have thus brought this rapid sketch to the close of the Tertiary, having omitted much of great interest, in order to direct attention to the one grand fact, — that the continent from the Potsdam sandstone, or before, to the Upper Tertiary, was one in its progress, — a single consecutive series of events according to a common law. It is seen, that the great system of oscillations, due to force pressing or acting from the southeast, which reached its climax in the rise of the Appalachians, then commenced a decline. We mark these oscillations still producing great results in the Jurassic Period along the whole eastern border from Nova Scotia to the Carolinas. Less effect appears in the Cretaceous Period, and gradually they almost die out as the Tertiary closes, leaving the Mississippi Valley and the eastern shores near their present level.

Thus were the great features of Middle and Eastern North America evolved; nearly all its grand physical events, including its devastations and the alternations in its rocks, were consequent upon this system of development.

Moreover, as I have observed, this system was some way connected with the relative positions of the continent and the oceanic basin, — meaning by the latter the profound depressions in which the oceans lie, and not including the shallow-water borders, which are only submerged portions of the Continent.

We need yet more definite knowledge of the Pacific border of North America to complete this subject. It is in accordance with the fact that the highest mountains are there, that volcanoes have been there in action; and also that, in the Tertiary Period, elevations of one to two thousand feet took place; and immediately before the Tertiary a still greater elevation of the Rocky Mountains across from east to west occurred. The system of changes between the Rocky Mountains and the Pacific has been on a grander scale than on the Atlantic border, and also from a different direction, — and this last is an element for whose influence on the general features we cannot yet make full allowance.

Through all this time, central British America appears to have taken little part in the operations; and what changes there were, except, it may be, in the Arctic regions, conformed to the system prevailing farther south, for the rocks of the Jurassic Age, like the Connecticut River sandstone, are found as far north as Prince Edward's Island, in the Gulf of St. Lawrence.

But the Tertiary Period does not close the history of the continent. There is another long Period, the Post-tertiary, — the period of the Drift, of the Mastodon and Elephant, of the lake and river Terraces, of the marine beds on Lake Champlain and the St. Lawrence, — all anterior to the Human Era.

From this time there is a fundamental change in the course of operations. The oscillations are from the north, and no longer from the southeast.

The *drift* is the first great event, as it underlies the other loose material of the surface; and all recognize it as a *northern* phenomenon, connected with northern oscillations.

The upper terrace of the lakes and rivers, and also the marine beds four hundred feet above the level of Lake Champlain, and five hundred above the St. Lawrence, which have been called Laurentian deposits, are marks of a *northern* depression, as no one denies.

The subsequent elevation to the present level again, by stages marked in the lower river terraces, was also *northern*, affecting the region before depressed.

The south felt but slightly these oscillations.

There are thus the following epochs in the Post-tertiary: — the *Drift Epoch;* the *Laurentian Epoch*, an epoch of depression; the *Terrace Epoch*, an epoch of elevation; — *three* in number, unless the Drift and Laurentian Epochs are one and the same.

As this particular point is one of much interest in American Geology, I will briefly review some of the facts connected with the drift.

The drift was one of the most stupendous events in geological history. In some way, by a cause as wide as the continent, — and, I may say, as wide nearly as the world, — stones of all sizes, to immense boulders of one to two thousand tons' weight, were transported, along with gravel and sand, over hills and valleys, deeply scratching the rocks across which they travelled. Although the ocean had full play in the many earlier ages, and an uneasy earth at times must have produced great convulsions, in no rock strata, from the first to the last, do we find imbedded stones or boulders at all comparable in magnitude with the immense blocks that were lifted and borne along for miles in the drift epoch.

Much doubt must remain about the origin of the drift, until the courses of the stones and scratches about mountain ridges and valleys shall have been exactly ascertained. The general course from the North is admitted, but the special facts proving or disproving a degree of dependence on the configuration of the land have not yet been sufficiently studied.

One theory, the most prevalent, supposes a deep submergence over New England and the North and West, even to a depth of four or five thousand feet, and conceives of icebergs as floating along the blocks of stone, and at bottom scratching the rocks. Another, that of the Professors Rogers, objects to such a submergence, and attributes the result to an incursion of the ocean from the north, in consequence of an earthquake movement beneath the Arctic Seas.

The idea of a submergence is objected to on the ground that the sea has left no proofs of its presence by fossils, or sea-shore terraces or beaches.

Unless the whole continent were submerged, of which there is no evidence whatever, there must have been in the Post-tertiary Period an east-and-west line of sea-shore, say across New Jersey, Pennsylvania, Southern Ohio, and the other States west, or still farther south; and yet no such sea-shore marks now exist to trace its outline, although the ocean must have been a portion of the same that had laid up the Cretaceous and Tertiary beds all along the coasts, and, in fact, already contained the oysters and clams, and many other species of Mollusks which now exist. Can it be, that, contrary to all the ways of the past, such a grand submergence as this view supposes, placing New England four thousand feet under water, could have transpired without a sea-shore record?

Very many have replied in the affirmative; and one able advocate of this view, who sees no difficulty in the total absence of sea-shore terraces or fossils at all levels above the Laurentian beds, finds in the succeeding epoch sea-shore accumulations in all the terraces of our rivers. Why this wonderful contrast? What withheld the waves from acting like waves in the former case, and gave unbounded license in the latter?

This much, then, seems plain, that the evidence, although negative, is very much like positive proof that the land was not beneath the sea to the extent the explanation of the drift phenomena would require.

There are other objections to this view of submergence. If North America were submerged from the southern boundary-line of the drift far into the Arctic regions, this would have made a much warmer climate for the continent than now; if only half-way, then there is another east-and-west shore line to be traced out, before the fact of the submergence can be admitted. Again, we know how the ice, while a glacier, or along a shore of cliffs, (for all bergs are believed to have once been glaciers,) may receive upon them, or gather up, heavy blocks of stone, even a thousand tons in weight, and bear them off to distant regions, as now happens in the Northern Atlantic. But we have no reason to believe that the massy foot of a berg could pick up such blocks and carry them twenty miles, to drop them again; and hence the short distance of travel would prove that the bergs were made that short distance to the north, and this implies the existence there of glacier valleys and requires a glacier theory.

But without considering other difficulties, I pass to the inquiry, Whether the lands, if not submerged, were at any higher level than now?

There is evidence of striking character, that the regions or coasts over the higher latitudes, in both the northern and southern hemispheres, were once much elevated above their present condition. The *fiords*, or deep coast channels, scores of miles long, that cut up the coast of Norway and Britain, of Maine, Nova Scotia, and Greenland, of Western America from Puget's Sound north, of Southern South America from Chiloe south, of Van Diemen's Land and other southern islands, are all valleys that could not have been scooped out when filled with the ocean's water as now; that could have been formed only when the land in those high latitudes, north and south, was elevated till their profound depths were nearly dry. Whether this elevation was in the period of the Post-tertiary has not been precisely ascertained. But as they are proof of a north-and-south system of oscillations, the same that was in action in the drift epoch, and as the cold that such a change would occasion is not very distinctly apparent in the Tertiary period, and much less in the earlier, we have reason for referring the greater part of the elevation to that drift era, and for believing that the excavation of these fiord valleys was then in progress. Both fiords and drift are alike high-latitude phenomena on all the continents, north and south. The change of climate between the Cretaceous and Tertiary, and the absence of Tertiary beds north of Cape Cod, may have been connected with an incipient stage in this high-latitude movement.

However this be, there is other evidence, in the cold of the drift period, of some extraordinary cause of cold. The drift in Europe and Britain is generally attributed to glaciers and icebergs during a period of greater cold than now; and the fact of this greater cold is so generally admitted, that it is common to speak of it as the glacial period. Professor Agassiz, moreover, has urged for this continent the glacial theory.

In a memoir of great research, by Mr. Hopkins, of Cambridge, England, the able author maintains that this glacial cold might have been produced over Europe, partly at least, by a diversion of the Gulf Stream from its present position. He seems in his paper to attribute too much effect to the Gulf Stream, and too little to the prevailing currents of the atmosphere; but, setting this aside, it is unfortunate for the hypothesis, that there is no reason to suppose that America was not then as much in the way of such a diversion as now. The small changes of level which the Tertiary and Post-tertiary beds of the Gulf have undergone, prove that the gate of Darien was early closed, and has since continued closed. America, as far as ascertained facts go, has not been submerged to receive the Stream over its surface. If it had been, it would have given other limits to her own drift phenomena; for it is an important fact that these limits in America and Europe show the very same difference in the climates or in the isothermals as that which now exists.

On the question of the drift, we therefore seem to be forced to conclude, whatever be the difficulties we may encounter from the conclusion, that the continent was not submerged, and therefore icebergs could not have been the main drift agents: the period was a cold or glacial epoch, and the increase of cold was probably produced by an increase in the extent and elevation of northern lands. Further than this, in the explanation of the drift, known facts hardly warrant our going.

If, then, the drift epoch was a period of elevation, it must have been followed by a deep submergence to bring about the depression of the continent, already alluded to, when the ocean stood four hundred feet in Lake Champlain, and a whale (for his bones have been found by the Rev. Z. Thompson of Burlington) was actually stranded on its shores; and when the upper terrace of the rivers was the lower river-flat of the valleys. This submergence, judging from the elevated sea-beaches and terraces, was five hundred feet on the St. Lawrence and Lake Champlain; eighty feet at Augusta, Maine; fifty feet at Lubec; thirty at Sancoti Head, Nantucket; over one hundred at Brooklyn, N. Y.; and two hundred to two hundred and fifty in Central New England, just north of Massachusetts; while south, in South Carolina, it was but eight or ten feet.

But whence the waters to flood valleys so wide, and produce the great alluvial plain constituting the upper terrace, so immensely beyond the capability of the present streams? Perhaps, as has been suggested for the other continent, and by Agassiz for this, from the melting snows of the declining glacier epoch. The frequent absence of fine stratification, so common in the material of this upper terrace, has often been attributed to a glacier origin.

According to this view, the events of the Post-tertiary Period in this country make a single consecutive series, dependent mainly on polar or high-latitude oscillations: — an elevation for the *first* or *Glacial Epoch;* a depression for the *second* or *Laurentian Epoch;* a moderate elevation again, to the present height, for the *third* or *Terrace Epoch.*

The same system may, I believe, be detected in Europe; but, like all the geology of that continent, it is complicated by many conflicting results and local exceptions; while North America, as I have said, is like a single unfolding flower in its system of evolutions.

There is the grandeur of nature in the simplicity to which we thus reduce the historical progress of this continent. The prolonged series of oscillations, acting by pressure from the southeast beneath the Atlantic, reach on through immeasurable ages, producing the many changes of level through the Silurian and Devonian, afterwards with greater frequency in the Carboniferous, and then, rising with quickened energy and power, folding the rocks and throwing up the long range of the Appalachians, with vast effusions of heat through the racked and tortured crust, next go on declining as the Jurassic and Cretaceous Periods pass, and finally fade out in the Tertiary. The northern oscillations, perhaps before in progress, then begin to exhibit their effects over the high temperate latitudes, and continue to the Human Era. The sinking of Greenland, now going on, may be another turn in the movement; and it is a significant fact, that, while we have both there and in Sweden northern changes of level in progress, such great secular movements have nowhere been detected on the tropical parts of the continents.

In deducing these conclusions, I have only stated in order the facts as developed by our geologists. Were there time for a more minute survey of details, the results would stand forth in bolder characters.

The sublimity of these continental movements is greatly enhanced when we extend our vision beyond this continent to other parts of the world. It can be no fortunate coincidence that has produced the parallelism between the Appalachian system and the grand feature lines of Britain, Norway, and Brazil, or that has covered the north and south alike with drift and fiords. But I will not wander, although the field of study is a tempting one.

In thus tracing out the fact, that there has been a plan or system of development in the history of this planet, do we separate the Infinite Creator from his works? Far from it: no more than in tracing the history of a planet. We but study the method in which Boundless Wisdom has chosen to act in creation. For we cannot conceive that to act without plan or order is either a mark of divinity or wisdom. Assuredly it is far from the method of the God of the universe, who has filled all nature with harmonies; and who has exhibited his will and exalted purpose as much in the formation of a continent, to all its details, as in the ordered evolution of a human being. And if man, from studying physical nature, begins to see only a Deity of physical attributes, of mere power and mathematics, he has but to look within at the combination of the affections with intellect, and observe the latter reaching its highest exaltation when the former are supreme, to discover proofs that the highest glory of the Creator consists in the infinitude of his love.

My plan, laid out in view of the limited time of a single address, has led me to pass in silence many points that seem to demand attention or criticism; and also to leave unnoticed the labors of many successful investigators.

There are some subjects, however, which bear on general Geology, that should pass in brief review.

I. The rock-formations in America may in general be shown to be synchronous approximately with beds in the European series. But it is more difficult to prove that catastrophes were synchronous, that is, revolutions limiting the ages or periods.

The revolution closing the Azoic Age, the *first* we distinctly observe in America, was probably nearly universal over the globe.

An epoch of some disturbance between the Lower and Upper Silurian is recognized on both continents. Yet it was less complete in the destruction of life on Europe than here, more species there surviving the catastrophe, and in this country there was but little displacement of the rocks.

The Silurian and the Devonian Ages each closed in America with no greater revolutions than those minor movements which divided the subordinate periods in those ages; Mr. Hall observes that they blend with one another, and the latter also with the Carboniferous, and that there is no proof of contemporaneous catastrophes giving them like limits here and in Europe.

But after the Carboniferous came the Appalachian revolution, one of the most general periods of catastrophe and metamorphism in the earth's history. Yet in Europe the disturbances were far less general than with us, and occurred along at the beginning and end of the Permian Period.

From this epoch to the close of the Cretaceous, there were no contemporaneous revolutions, as far as we can discover. But the Cretaceous Period terminates in an epoch of catastrophe which was the most universal on record, all foreign Cretaceous species having been exterminated, and all American, with a few doubtful exceptions. This third general revolution was the prelude to the Mammalian Age. But there is no time to do this subject justice, and I pass on, — merely adding, on account of its interest to those who would understand the first chapter of Genesis, that there is no evidence whatever in Geology, that the earth, after its completion, passed through a chaos and a six days' creation at the epoch immediately preceding man, as Buckland, in the younger days of the science, suggested, on *Biblical*, not on geological, ground. No one pretends that there is a fact or hint in Geology to sustain such an idea: moreover, the science is utterly opposed to it.

II. The question of the existence of a distinct *Cambrian system* is decided adversely by the American records. The Mollusca in all their grand divisions appear in the Lower as well as Upper Silurian, and the whole is equally and alike the Molluscan or Silurian Age. The term Cambrian, therefore, if used for fossiliferous strata, must be made subordinate to Silurian.

The *Taconic system* of Emmons has been supposed by its author to have a place inferior to the Cambrian of Sedgwick, or else on a level with it. But the investigations of Hall, Mather, and Rogers, and more lately of Logan and Hunt, have shown that the Taconic slates belong with the upper part of the Lower Silurian, being, in fact, the Hudson River shales, far from the bottom of the scale.

III. The American rocks throw much light on the origin of coal. Professor Henry D. Rogers, in an able paper on the American Coal-fields, has well shown that the condition of a delta or estuary for the growth of the coal-plants, admitted even now by some eminent geologists, is out of the question, unless the whole continent may be so called; for a large part of its surface was covered with the vegetation. Deltas exist where there are large rivers; and such rivers accumulate and flow where there are mountains. How, then, could there have been rivers, or true deltas of much size, in the Coal Period, before the Rocky Mountains or Appalachians were raised? It takes the Andes to make an Amazon. This remark has a wider application than simply to the Coal Era.

IV. In this connection, I add a word on the idea that the rocks of our continent have been supplied with sands and gravel from a continent now sunk in the ocean. No facts prove that such a continent has ever existed, and the whole system of progress, as I have explained, is opposed to it. Moreover, gravel and sands are never drifted away from seashores, except by the very largest of rivers, like the Amazon; and with these, only part of the lightest or finest detritus is carried far away; for much the larger part is returned to the coast through tidal action, which has a propelling movement shoreward, where there are soundings. The existence of an Amazon on any such Atlantic continent in Silurian, Devonian, or Carboniferous times, is too wild an hypothesis for a moment's indulgence.

V. The bearing of the facts in American Palæontology on the science, might well occupy another full discourse. I will close with brief allusions to some points of general interest.

1. The change in the Fauna of the globe as the Age of Man approaches, is one of the most interesting facts in the earth's history. It was a change not in the types of the races, (for each continent retains its characteristics,) but a remarkable dwindling in the size of species. In North America the Buffalo became the successor to the huge Mastodon, Elephant, and Bootherium; the small Beaver to the great Castoroides; and the existing Carnivora are all comparatively small.

Parallel with this fact, we find that in South America, as Dr. Lund observes, where, in the last age before Man, there were the giant Megatherium and Glyptodon, and other related Edentates, there are now the small Sloths, Armadillos, and Ant-eaters.

So, also, on the Oriental continent, the gigantic Lion, Tiger, Hyena, and Elephant, and other monster quadrupeds, have now their very inferior representatives.

In New Holland, too, the land of Marsupials, there are Marsupials still, but of less magnitude.

2. This American continent has contributed to science a knowledge of some of the earliest traces of Reptiles, — the species of the Pennsylvania coal formation, described by Mr. King and Mr. Lea, and others from the Nova Scotia coal-fields, discovered by Messrs. Dawson and Lyell.

It has afforded the earliest traces of birds thus far deciphered in geological history, — the colossal and smaller waders, whose tracks cover the clayey layers and sandstone of the Jurassic rocks in the Connecticut valley. The earliest

Cetacea yet known are from the American Cretaceous beds, as described by Dr. Leidy. And among the large Mammals which had possession of the renewed world after the Cretaceous life had been swept away, the largest, as far as has been ascertained, lived on this continent. The Palæotheria of the Paris Basin, described by Cuvier, were but half the size of those of Nebraska.

But here our boasting ceases, for, as Agassiz has shown, the present Fauna of America is more analogous to the later Tertiary of Europe than to the existing species of that continent.

In the Palæozoic Ages, to the close of the Coal Period, the American continent was as brilliant and profuse in its life as any other part of the world. It was a period, indeed, when the globe was in an important sense a unit, not individualized in its climates or its distribution of life, and only partially in its seas. But from this time the contrast is most striking.

The whole number of known American species of animals of the Permian, Triassic, Jurassic, Cretaceous, and Tertiary Periods is about two thousand; while in Britain and Europe, a territory even smaller, there were over twenty thousand species. In the Permian we have *none*, while Europe has over two hundred species. In the Triassic, *none;* Europe, one thousand species. In the Jurassic, sixty; Europe, over four thousand. In the Cretaceous, three hundred and fifty to four hundred; Europe, five to six thousand. In the Tertiary, hardly fifteen hundred; Europe, about eight thousand.

America, since Palæozoic times, has therefore been eminent for the poverty of its Fauna.

Again: the Mammalian Age in America, although commencing with huge Pachyderms, shows little progress afterward. The larger quadrupeds continue to be mainly herbivorous, and the Carnivora, the higher group, are few and of comparatively small size. *The Herbivora are still the typical species.* While in Europe and Asia, at the same time, — that is, in the Post-tertiary, — the Carnivora are of great size and ferocity, far exceeding the largest of modern Lions and Tigers. The single species of Lion described, from a bone from near Natchez, by Dr. Leidy, hardly lessens the contrast.

South America, as has been remarked by Agassiz and others, sustains this inferior position of America. The huge Sloths, Megatheria, and other Edentata of the South, are even lower in grade than the ordinary Herbivora, and place that Southern continent at an inferior level in the scale. Although there were Carnivora, they were much smaller than the European. *The Edentates are*, in fact, *its typical species.*

The supremacy of the great Oriental continent is, therefore, most signally apparent.

The contrast is still greater with Australia and New Zealand, whose past and present Fauna and Flora have been well said by Agassiz and Owen to represent the Jurassic Period, — the present era affording Trigonias, Terebratulæ, Cestraciont Fishes, and the Araucarian Coniferæ, all Jurassic types, besides Kangaroos and Moas. Among Mammals, *the Marsupials*, the lowest of all in the class, *are its typical species.*

Ever since Palæozoic times, therefore, the Oriental Continent, — that is, Europe, Asia, and Africa combined, — has taken the lead in animal life. Through the Reptilian Age, Europe and Asia had species by thousands, while America was almost untenanted. In the later Mammalian Age, North America was yet in the shade, both in its Mammals and lower tribes; South America in still darker shadows; and Australia even deeper still. The earth's antipodes were like light and darkness in their zoölogical contrasts. And was there not in all this a prophetic indication, which had long been growing more and more distinct, that the Eastern Continent would be man's chosen birthplace? that the long series of living beings, which had been in slow progression through incalculable ages, would there at last attain its highest exaltation? that the stupendous system of nature would there be opened to its fullest expansion?

Another of our number has shown in eloquent language how the diversified features and productions of the Old World conspired to adapt it for the childhood and development of the race; and that, when beyond his pupilage, having accomplished his rescue from himself and the tyranny of forces around him, and broken the elements into his service, he needed to emerge from the trammels of the school-house in order to enjoy his fullest freedom of thought and action, and social union. Professor Guyot observes further, that America, ever free, was the appointed land for this freedom and union, — of which its open plains, and oneness of structure, were a fit emblem; and that, although long without signs of progress or hope in its future, this land is to be the centre of hope and light to the world.

In view of all these arrangements, man may well feel exalted. He is the last of the grand series. At his approach, the fierce tribes of the earth drew back, and the race dwindled to one fourth its bulk and ferocity, — the huge Mastodons, Lions, and Hyenas yielding place to other species, better fit to be his attendants, and more in harmony with the new creation.

Partaking of the Divine image, all nature pays him tribute; the universe is his field of study; an eternity his future. Surely it is a high eminence on which he stands.

But yet he is only *one* of the series; one individuality in the vast system. How vain the philosophy which makes the creature the God of nature, or nature its own author! Infinitely beyond man, infinitely beyond all created things, is that Being with whom this system, and the combined systems of immensity, were as one purpose of His will.

REPORT

ON THE RECENT PROGRESS

OF

ORGANIC CHEMISTRY.

BY DR. WOLCOTT GIBBS.

In attempting to trace the recent progress of Organic Chemistry, there are two points of view from which the subject may with propriety be considered. Either we may confine our attention to the directly positive additions which have been made to our knowledge by the discovery and description of new compounds, or we may consider the true progress of the science to be marked by the development of general laws, and by the birth of leading ideas prolific in new facts. As the term Organic Chemistry is one which is somewhat loosely applied, and which is often made to include the Chemistry of Animal and Vegetable Physiology, it is proper to state that I here employ it in a more limited sense, and that I understand by it that branch of the science which treats of the immediate chemical products of the vital force, as well as of those bodies which are derived, or which may be considered as derived, from these by the processes of the laboratory. It is from the second of the two points of view mentioned above that I propose to consider my subject, and, viewed from this point, particular compounds will be of interest only so far as they serve to illustrate general principles. Four leading theories have, as I think, exerted the most powerful influence upon the progress of Organic Chemistry during the last ten years. They are the theories of Compound Radicals, of Conjugation, of Homologues, and of Substitutions. For the sake of distinctness, I shall consider these separately, and shall endeavor to refer to each the facts which may fairly be considered as flowing from it, without, of course, implying that in every case the theory has preceded the discovery of the fact. And I shall conclude by presenting a brief sketch of the present condition of the science, and of the questions upon which the opinions of chemists are now divided.

The theory of Compound Radicals, as is well known, was first proposed by Berzelius, in the year 1817, and was supported by him during his subsequent career with the tenacity of conviction and the earnestness which so strikingly marked his scientific character. It was adopted and vigorously supported by Liebig, who, sixteen years later, in conjunction with Wöhler, studied with remarkable success the Benzoyl series, and applied the theory in a new form, by assuming the existence of radicals containing oxygen. Berzelius rejected this view, and, limiting the conception of a compound radical to bodies composed of carbon and hydrogen, introduced the Ethyl theory, and applied it to alcohol and to the ethers which were at that time known. The Ethyl theory, speedily adopted by the German chemists, was carried by Liebig's pupils to England, and even met with a favorable reception from several French chemists of note. In France, however, it soon fell into disfavor. Laurent endeavored to substitute for it his theory of Cores, while others, denouncing compound radicals as purely hypothetical bodies, offered other hypotheses of their own, which had at least the merit of being French. Thus, in a recent treatise, the ethers are derived from carburets of hydrogen homologous with marsh gas, an equivalent of hydrogen being replaced by an equivalent of oxygen plus an equivalent of anhydrous acid. Not a single case is, however, cited in which such a substitution has been actually effected, and the homologues of marsh gas, at the time the work in question was written, were as purely hypothetical as the compound radicals themselves. In other cases the theory was ingeniously avoided by using the term "residue" in place of radical. Thus in benzamid $NH_2, C_{14}H_5O_2$, it was said that the residue $C_{14}H_5O_2$ replaced an equivalent of hydrogen in the ammonia, the reaction being represented by the equation $NH_3 + C_{14}H_5O_2, Cl = HCl + NH_2, C_{14}H_5O_2$. In like manner the radicals NO_4 and SO_2 were termed the residues of nitric and sulphuric acid. The investigations of Dr. Hofmann on the ammonias and ammoniums produced an immediate change in the views even of those chemists who had rejected the theory of Compound Radicals as involving unnecessary and uncalled-for hypotheses. These researches established the important fact, that in ammonium, NH_4, 1, 2, 3, or 4 equivalents of hydrogen may be replaced by an equal number of equivalents of a zincous or electro-positive radical, like methyl or ethyl; and not merely this, but that four different radicals may be present in an ammonium, and three in an ammonia. Ammonia and ammonium became at once generic, instead of specific, terms. The discovery of Ethylamin and Methylamin by Wurtz had already established the existence of highly volatile ammonias, which could be regarded theoretically as ammonia in which an equivalent of hydrogen is replaced by an equivalent of ethyl or methyl, and this view was actually taken by Wurtz. But the mode of formation of the new alkalies employed by him did not, in itself, suggest the idea of such a replacement, even in the case of a single equivalent of hydrogen. While, therefore, we owe to Wurtz an isolated, though beautiful and fruitful discovery, we must accord to Hofmann the merit, not merely of one of the finest generalizations in the science, but also of one of the most powerful methods of studying internal molecular structure which we possess. Hofmann's memoir was published in the Philosophical Transactions of the Royal Society in 1850. In it he clearly established these facts:—

First. That in either ammonia or ammonium any number of equivalents of hydrogen may be replaced by an equal number of equivalents of a compound radical.

Second. That the order of replacement is indifferent.

Third. That the new ammonias or ammoniums so formed are really such; or, in other words, that the distinctive properties of the primitive substance are not altered by the successive replacements of the hydrogen, excepting only in degree.

The following formulas will serve to illustrate the character of Hofmann's discoveries:—

Ammonias.

Ammonia . . .	$N.HHH$
Ethylamin . .	$N.HH.C_4H_5$
Diethylamin . .	$N.H.C_4H_5.C_4H_5$
Triethylamin .	$N.C_4H_5.C_4H_5.C_4H_5$
Ethylmethylphenylamin	$N.C_2H_3.C_4H_5.C_{12}H_5$.

Ammoniums.

Chloride of Ammonium . .		$N.HHHH, Cl$
"	Methyl Ammonium	$N.C_2H_3.HHH, Cl$
"	Dimethyl Ammonium	$N.C_2H_3.C_2H_3.HH, Cl$
"	Trimethyl Ammonium	$N.C_2H_3.C_2H_3.C_2H_3.H, Cl$
"	Tetramethyl Ammonium	$N.C_2H_3.C_2H_3.C_2H_3.C_2H_3, Cl$

Ethyl methyl amyl phenyl ammonium Chloride $N.C_4H_5.C_2H_3.C_{10}H_{11}.C_{12}H_5, Cl$

Hofmann's discovery left chemists no alternative. It was of no avail to say that methyl and ethyl, for example, are only conjugates of hydrogen, C_2H_2, H and C_4H_4, H, and therefore that the radical theory is not proved by Hofmann's discoveries. For the radical theory makes no assumptions as to the internal molecular structure of the radicals themselves, but only assumes that C_4H_5 enters into combination as a whole, and as such replaces hydrogen equivalent for equivalent. When iodide of ethyl, by acting upon ammonia, produces Ethylamin and iodide of hydrogen, there is as real a substitution of ethyl for hydrogen as there is of chlorine for hydrogen when chlorine acts upon acetic acid. In both cases a binary molecule is presented to a single molecule of hydrogen; one atom of the binary molecule (dyad of Laurent) unites with one of hydrogen, while the other takes the place of the hydrogen in the combination. We must therefore admit, that, whatever be the true constitution of C_4H_5, the compound C_4H_5 | I acts upon a body containing hydrogen precisely as Cl | Cl does, and C_4H_5 is therefore equivalent to Cl, and consequently to H or one equivalent of hydrogen. This is all that the radical theory, as applied at least to ethyl and its compounds, can demand, and a general, though not always cordial, acquiescence in the theory in question has taken the place of the old hostility. It deserves to be mentioned in this place, that ten years before Wurtz's discovery of ethylamin and methylamin, Liebig had foreseen the possible existence of an ammonia in which an equivalent of hydrogen is replaced by one of ethyl, and had asserted that such a body would be analogous to ordinary ammonia, and would possess basic properties. Time has seldom given a more beautiful verification of the predictions of genius.

Hofmann's method of replacing hydrogen in the ammonias and ammoniums has since been frequently employed, not merely in studying the molecular structure of the natural alkaloids by determining how many equivalents of replaceable hydrogen they contain, but also in producing substitutions of hydrogen in other bodies not alkaline in their character. In this manner many new alkaloids have been formed, and a prospect, ever brightening, has been opened of artificially producing in the laboratory some of the most valuable therapeutic agents which we possess.

An important addition to the radical theory has recently been made by Cloez and by Natanson, who have shown that an equivalent of hydrogen in ammonia can be replaced by an equivalent of Formyl, C_2H, or of Acetyl, C_4H_3, radicals assumed by Berzelius in formic and acetic acids, and whose existence, like that of Ethyl and Methyl, has been stoutly denied. The new ammonias, which we may call Formylamin and Acetylamin, are less strongly basic than ordinary ammonia, or than the corresponding Ethylamin and Methylamin. This arises from the more highly electro-negative character of Formyl and Acetyl, and the discovery at once clears up all doubts as to the constitution of the extensive class of acids homologous with formic acid, and shows that they are the oxides of electro-negative radicals. It will be remembered, that while the highly electro-positive metals, like sodium and zinc, form with oxygen only protoxides which are basic, the more electro-negative metals, like chromium, iron, and manganese, form acid as well as basic oxides. Now, as Zinin has recently shown that Propionyl, C_6H_5,— and therefore by analogy its homologue, Acetyl, — forms a basic protoxide as well as an acid teroxide, we see that Ethyl is to Acetyl what zinc is to iron. Our simple radicals are, then, perfectly represented by their compound analogues. In these cases, also, the radical theory makes no hypothesis with respect to the true molecular structure of the radicals themselves, nor does it assert that all three equivalents of oxygen in any one of these organic acids are present in exactly the same form, since, as in sulphuric acid, one equivalent may be combined in a different manner from the other two, and may be capable of replacement by an equivalent of chlorine or other electro-negative body. The Ethyl and Acetyl theories may then be considered as demonstrated, since they furnish the only means of explaining the fact of the equivalent replacement of hydrogen. But while the researches of Hofmann, and those which have flowed from them, have established the truth of the Berzelian theory of compound radicals as applied to compounds of carbon and hydrogen, they have also shown that the extension of Berzelius's views by Liebig and Wöhler must be received, and that we must admit the existence of radicals containing oxygen. As a supplement to Hofmann's discoveries, Gerhardt has proved that 1, 2, or 3 equivalents of hydrogen in ammonia may be replaced by an equal number of equivalents of a compound radical containing oxygen, as, for example, $C_{14}H_5O_2$, the Benzoyl of Liebig and Wöhler. Thus we have ammonia NH_3, Benzamid $NH_2C_{14}H_5O_2$, Dibenzamid $NH(C_{14}H_5O_2)_2$, and Tribenzamid $N(C_{14}H\ O_2)_3$. The Benzoyl theory in its original form is thus demonstrated, and it is not only clearly shown that Berzelius was in error in rejecting compound radicals containing oxygen, but that in many substances we must admit the existence of a secondary as well as of a primitive radical. Thus the empirical formula of acetic acid is $C_4H_4O_4$, while its rational formula, if we admit that it contains water, is $C_4H_3O_2.O+HO$, just as anhydrous sulphuric acid is the oxide of the radical SO_2, and anhydrous nitric acid the oxide of NO_4. Hence in acetic acid the primitive radical is C_4H_3, and the secondary radical $C_4H_3O_2$. Kolbe has proposed to give to the names of those radicals which contain oxygen the termination "oxyl," to distinguish them from primary radicals consisting only of carbon and hydrogen, and for which the termination "yl" has long been in use. This suggestion deserves general adoption. In accordance with it we shall have acetyl C_4H_3, and acetoxyl $C_4H_3O_2$, benzoyl $C_{14}H_5$, and benzoxyl $C_{14}H_5O_2$, sulphur S, and sulphuroxyl SO_2, nitrogen N, and nitroxyl NO_4.

The discovery of Gerhardt stands in the same relation to that of Liebig and Wöhler which that of Hofmann bears to the discovery of Ethylamin by Wurtz. Thus, Benzamid, Dibenzamid, and Tribenzamid, are parallel to Ethylamin, Diethylamin, and Triethylamin. In any triamid the three equivalents of hydrogen of the primitive ammonia may be replaced by three *different* radicals containing oxygen. No replacement of even a single equivalent of hydrogen in ammonium by a radical containing oxygen has yet been observed, but perhaps the natural alkaloids may furnish instances of this. It may further be remarked, that the basic property of ammonia is lost by the substitution even of a single equivalent of hydrogen by a radical containing oxygen; the electro-negative character of the latter impressing itself upon the whole compound, rendering the existence even of acid ammonias not improbable. I shall have occasion to return to this subject.

In connection with the theory of compound radicals, I may properly, as it appears to me, mention the anhydrous organic acids, the discovery of which we also owe to Gerhardt. By the action of chloride of benzoxyl on benzoate of potash, Gerhardt obtained anhydrous benzoic acid, the formation of which may be represented by the equation

$$C_{14}H_5O_2Cl + KO, C_{14}H_5O_3 = 2\ C_{14}H_5O_3 + KCl.$$

Similar reactions have yielded a large number of anhydrous acids, while, by using the chlorides of radicals different from those contained in the potash salts employed, Gerhardt and Chiozza have obtained many new double acids or compounds of two organic acids with each other. These, as a general rule, have very little stability, and are easily separated into their components. Water converts the anhydrous organic acids more or less rapidly into the ordinary hydrates.

Gerhardt's beautiful discovery followed quickly upon a very similar one by Williamson, who, by the action of the iodides of the ethyl series upon potash or soda alcohol, obtained a series of new substances, which may be regarded as alcohols, in which an equivalent of hydrogen is replaced by an equivalent of methyl, ethyl, &c. Thus,

$$C_4H_5O,\ KO + C_4H_5I = 2\ C_4H_5O + KI;$$
$$C_4H_5O,\ KO + C_2H_3I = C_4H_5O,\ C_2H_3O + KI.$$

From these reactions Williamson infers that the equivalent of ether is twice as high as that hitherto adopted, and refers both the ethers and alcohols to the type of water with a double equivalent, O_2H_2. On this view alcohol is water in which one equivalent of hydrogen is replaced by one of ethyl, while ether is water in which both equivalents of hydrogen are replaced by ethyl. Thus we have, $O_2 \left\{ \begin{matrix} H \\ H \end{matrix} \right.$ water, $O_2 \left\{ \begin{matrix} H \\ C_4H_5 \end{matrix} \right.$ alcohol, $O_2 \left\{ \begin{matrix} C_4H_5 \\ C_4H_5 \end{matrix} \right.$ ether, $O_2 \left\{ \begin{matrix} C_2H_3 \\ C_4H_5 \end{matrix} \right.$ methyl-ethyl alcohol.

Gerhardt has extended these views to the organic acids, both hydrous and anhydrous, considering, for example, ordinary acetic acid to be $O_2 \left\{ \begin{matrix} H \\ C_4H_3O_2 \end{matrix} \right.$, while anhydrous acetic acid is $O_2 \left\{ \begin{matrix} C_4H\ O_2 \\ C_4H_3O_2 \end{matrix} \right.$, and aceto-benzoic acid $O_2 \left\{ \begin{matrix} C_4H_3O_2 \\ C_{14}H_5O_2 \end{matrix} \right.$.

These views, which are much strengthened by the comparison of the physical properties of the alcohols, ethers, and acids, have found many advocates; but as they can scarcely be said as yet to have exercised a positive influence upon the progress of organic chemistry, they do not require further notice in this place. The alcohols discovered by Williamson have not yet been carefully studied. We know only their boiling points and the densities of their vapors, but of their chemical properties and relations we are wholly ignorant. Thus common alcohol yields by oxidation, or better by replacement of hydrogen by oxygen, acetic acid; what is then the corresponding product in the case of methyl-ethyl alcohol, $C_4H_6O + C_2H_3O$? Perhaps either acetate of methyl, or formate of ethyl, $C_4H_3O_3 + C_2H_3O$, or $C_2HO_3 + C_4H_5O$. Before we can arrive at the true theory of the constitution of these and similar bodies, a thorough study of the products of their decomposition, as well as of their general chemical relations, is necessary. In the isolation of the organic radicals some progress has been made, though it is in many cases, to say the least, doubtful whether the substances isolated are the radicals themselves, or only bodies isomeric with them. By the electrolysis of acetate and valerate of potash, Kolbe has obtained bodies having the empirical formulas C_2H_3 and C_8H_9, and which he regards as methyl and valyl. In like manner, by the action of metallic zinc upon iodide of ethyl, as well as by the decomposition of this latter body by light, Frankland has obtained the body C_4H_5, which is either identical or isomeric with ethyl. The chemists of the French school double the equivalents of all these bodies, and regard them as corresponding to two volumes of vapor like free hydrogen, of which, with Berzelius, they consider atom and volume to be synonymous. Thus, with them, Kolbe's methyl is $\left\{ \begin{matrix} C_2H_3 \\ C_2H_3 \end{matrix} \right.$, corresponding to $\left\{ \begin{matrix} H \\ H \end{matrix} \right.$, the equivalent of hydrogen being one half, if we consider that of oxygen to be eight. The only difference, therefore, between the French and German chemists is this: that while Kolbe asserts that methyl is isolable with the equivalent which it has in iodide of methyl, Gerhardt maintains that the isolated ethyl has an equivalent which is twice as high. On this view, which is strongly supported by the relations between the boiling points and atomic volumes of the bodies in question, free ethyl is to ethyl in combination what free hydrogen is to hydrogen in combination. It will be seen at once, that the question is here the same as in the case of the ethers and organic acids above mentioned. Whichever view be adopted, it cannot, I think, be reasonably doubted that many compound radicals have actually been isolated in the same sense as hydrogen itself. The theory can demand no more than this. If we consider a theory to be only a conception which enables us to classify, arrange, and bring under a single point of view, a great and connected series of well-ascertained facts, then I maintain that the chemical theory of compound radicals is as perfect as any theory in the whole range of the physical sciences.

I pass, in the next place, to the consideration of the theory of Conjugation, if I may be allowed to use the term, understanding by it the union of a body, A, with another body, B, of such a nature that the properties of B are thereby changed in degree, but not in kind. We have here to distinguish three different forms or cases, — conjugate radicals, acids, and bases. Of these three classes of compounds the conjugate radicals, which, in fact, are the primitives of the other two, have been most carefully studied, and deserve the most attention. The starting-point of all investigations of this subject was the celebrated memoir of Bunsen on Kakodyl, a body which has the empirical formula C_4H_6As, but which is now recognized as a conjunct of two equivalents of methyl with one of arsenic, and which has the rational formula $2\ C_2H_3, As$.

As a type of the simplest class of metallic conjugate radicals we may take Stannethyl, or ethyl-tin, as I should prefer to call it, the discovery of which is due to Frankland, and which has the formula C_4H_5,Sn. It is a colorless, oily liquid, which oxidizes in the air, forming a protoxide, $C_4H_5 . SnO$, which acts as a powerful base, and forms very well defined crystalline salts. The reaction by which this body is formed is represented by the equation $C_4H_5I + 2\ Sn = SnI + C_4H_5 . Sn$; and by a similar reaction aided by heat or light, Frankland has succeeded in preparing other conjugate radicals of analogous constitution. But the type of ethyl-tin is by no means the only or the most interesting one. By the action of iodide of ethyl upon alloys of tin, lead, antimony, arsenic, and bismuth, with potassium or sodium, Löwig has prepared a large number of new radicals, many of which are of very complex constitution, but which may all be considered as composed of *m* equivalents of an element united to *n* equivalents of an organic radical so as to form a conjugate metal.

As antimony, arsenic, and bismuth belong to the same group with nitrogen, it was natural to suppose that the compounds of these metals with three equivalents of ethyl, or of another organic radical, would resemble in chemical properties the ammonias of Hofmann, to which we have already alluded, since $As\ (C_4H_5)_3$ and $Sb\ (C_4H_5)_3$ have the same molecular type as NH_3 and $N\ (C_4H_5)_3$. This, however, is not the case; for while ammonia and its derivatives never unite directly with oxygen, chlorine, &c., the radicals $Sb\ (C_4H_5)$ and $As\ (C_4H_5)_3$ form oxides, chlorides, &c., like the metals themselves, and we have such compounds as $Sb\ (C_4H_5)_3\ O_2$, $As\ (C_4H_5)_3\ S_2$, &c. In like manner, when ammonia, NH_3, is brought into contact with chlorhydric acid, direct combination ensues, and we have chloride of ammonium formed; when, however, $Sb\ (C_4H_5)_3$ is treated with the same acid, hydrogen is evolved, and we have the chloride of the radical $Sb\ (C_4H_5)_3$, the reaction being represented by the equation $Sb\ (C_4H_5)_3 + 2\ HCl = Sb\ (C_4H_5)_3\ Cl_2 + 2\ H$. From this it is clear that the bodies Sb $(C H_5)_3$ and As $(C_4H_5)_3$ are not ammonias, but conjugate metals, and their formulas may therefore be written $(C_4H_5)_3$ Sb and $(C_4H_5)_3$ As.

It is easy to show, however, that even in these cases the ammonia type is not completely lost. If we add iodide of ethyl to triethyl-antimony, combination ensues, and we have the iodide of a true ammonium, Sb $(C_4H_5)_4$ I, in which antimony replaces nitrogen, and ethyl replaces hydrogen. The new radical Sb $(C_4H_5)_4$ unites with a single equivalent of oxygen to form a powerful base yielding well-crystallized salts, while, like other ammoniums, it does not appear to be isolable, and in this particular differs essentially from the conjugate radicals, most of which can be obtained in the free state. It thus appears that the function of the body Sb $(C_4H_5)_3$ is a double one, and that it may play the part of an ammonia as well as of a radical. As a radical, we regard it as antimony coupled with three equivalents of ethyl, which modify the chemical properties and relations of the metal, without, however, so far changing them that they can no longer be recognized.

Frankland has recently directed attention to an extremely remarkable fact in connection with these compounds. Nitrogen, phosphorus, arsenic, antimony, and bismuth are all capable of uniting with five equivalents of oxygen or chlorine as a maximum, while tin unites at most with two, and zinc with one. Now, the radicals Sb $(C_4H_5)_3$ and As $(C_4H_5)_3$ unite with two equivalents of oxygen or chlorine, the radical Sb $(C_4H_5)_4$ with one, the radical As $(C_4H_5)_2$ with three, the radical $C H_5 . Sn$ with one, while finally the body $C_4H_5 . Zn$ forms no compound with oxygen and chlorine, but is completely decomposed by these bodies, yielding ether and oxide or chloride of zinc. Frankland suggests, that in all these cases the original molecular type of the highest oxide is preserved, so that in the new conjugate radicals ethyl may be regarded as replacing oxygen. Thus we have, —

Oxide of zinc	ZnO
Zinc ethyl	$Zn\,(C_4H_5)$
Peroxide of tin	$SnOO$
Stannethyl oxide	$Sn\,.\,C_4H_5\,.\,O$

Antimonic Acid $$Sb\left\{\begin{array}{l}O\\O\\O\\O\\O\end{array}\right.$$

Oxide of Stibethylium $$Sb\left\{\begin{array}{l}O\\O_4H_5\\O_4H_5\\O_4H_5\\C_4H_5\end{array}\right.$$

Oxide of Tristibethyl $$Sb\left\{\begin{array}{l}O\\O\\C_4H_5\\C_4H_5\\C_4H_5\end{array}\right.$$

Oxide of Kakodyl $$As\left\{\begin{array}{l}O\\O\\O\\C_2H_3\\C_2H_3\end{array}\right.$$

Should a radical having the formula $Sb\,.\,C_4H_5$ or $As\,.\,C_4H_5$ be hereafter discovered, it ought to unite with four equivalents of oxygen or chlorine. Frankland's view is certainly a most ingenious and suggestive one, and it is impossible to deny that a molecular uniformity of some kind exists in the cases mentioned.

The laborious and patient investigations of Löwig have, however, made us acquainted with a large number of conjugate radicals, the constitution of which is by no means so simple, and which we are at present unable to classify in a perfectly satisfactory manner. Thus we have such compounds as

$Sn_2\,(C_4H_5)_2$	$Sn_2\,(C_4H_5)_3$
$Sn_4\,(C_4H_5)_4$	$Sn_4\,(C_4H_5)_5$
$Sn_4\,(C_4H_5)_3$	&c., &c.

All these radicals combine with one equivalent of oxygen to form basic oxides. Löwig has ingeniously proposed to consider these bodies as corresponding to the ordinary organic radicals composed of carbon and hydrogen, the tin being supposed to replace carbon, and the ethyl to replace hydrogen.

Thus the body $Sn_4\ (C_4H_5)_5$ would be ethyl in which carbon is replaced by tin and hydrogen by ethyl. This view is certainly a bold one, but it has hitherto met with but little favor, though no other theory has been proposed which can take its place. Frankland's view, of course, does not apply to radicals like these.

The conjugate acids have not as yet been very thoroughly studied. In many cases of such acids the radicals themselves have been isolated, as, for example, in kakodylic acid. The researches of Kolbe have, however, made us acquainted with many interesting bodies belonging to this class, and have rendered it probable that the class itself is a very numerous one. Thus the compounds having the formulas $C_2H\ .\ S_2O_5 + HO$, $C\ H_5\ .\ S\ O_5 + HO$, &c., may be regarded as dithionic acid, $S_2O_5 + HO$, coupled with methyl and ethyl.

The Berzelian theory of the constitution of the organic alkaloids, that they are conjugate ammonias, morphine, for instance, $C_{36}H_{19}NO_6\ .\ NH_3$, was never very extensively adopted, and fell to the ground with Hofmann's discovery of the replaceability of hydrogen in ammonia and ammonium by complex radicals. All the alkaloids have since been considered as substituted ammonias or ammoniums, perhaps rather too hastily, since the Berzelian view may still be true in particular cases. Genth's discovery of two bases, in which ammonia acts as the couplet and sesquioxide of cobalt as the body coupled, as well as the subsequent observation, by the writer of this report, of similar bases containing deutoxide of nitrogen, will give, it is hoped, a new impulse to the study of this class of bodies. I may, perhaps, without impropriety, so far anticipate the publication of the results of the joint investigation of Genth and myself, as to state the constitution of some of these bases, more particularly as I have already — at the Cleveland meeting — communicated to the Association a brief account of the more remarkable compounds.

The bases in question may be considered as oxides, chlorides, &c. of conjugate radicals formed by the union of two equivalents of cobalt with a certain number of equivalents of ammonia, or of ammonia and deutoxide of nitrogen. Adopting, with a slight modification, and extending the nomenclature of Frémy, we have the following radicals with the bases resulting from their combination with oxygen: —

	Radicals.	Bases.
Roseocobalt	$5\,NH_3 . Co_2$	$5\,NH_3 . Co_2O_3$ triacid.
Luteocobalt	$6\,NH_3 . Co_2$	$6\,NH_3 . Co_2O_3$ "
Purpureocobalt	$5\,NH_3 . Co_2$	$5\,NH_3 . Co_2O_3$ uniacid.
Xanthocobalt	$NO_2 . 5\,NH_3 . Co_2$	$NO_2 . 5\,NH_3 . Co_2O_3$ "
Dixanthocobalt	$2\,NO_2 . 5\,NH_3 . Co_2$	$2\,NO_2 . 5\,NH_3 . Co_2O_3$ biacid.

The chlorides of these bases correspond in constitution to the oxides, and form very extensive and well-defined series of double salts with the chlorides of the more electro-negative metals. It is worthy of notice, that while the oxygen bases, whose formulas are written above, may be regarded simply as conjugates of the sesquioxide of cobalt, the sesquioxide itself does not always retain its property of combining with three equivalents of acid. The saturating capacity of the base depends, therefore, in some measure at least, on the nature of the couplet. None of the above assumed radicals have been isolated, nor does it appear probable that they are capable of existing in a free state. The existence of two allotropic forms or modifications of cobalt is rendered probable by the discovery of Purpureocobalt, the constitution of which is identical with that of Roseocobalt, while not merely the crystalline forms of the corresponding compounds, but all the reactions of the two bases, are entirely distinct. Two allotropic forms of the protoxide of cobalt have already been observed by Genth.

Since the discovery of the ammonia-cobalt bases, Claus has obtained ammonia-rhodium and ammonia-iridium bases corresponding to Roseocobalt, and it cannot be doubted that further researches will add greatly to the number of similar compounds. It is certainly very remarkable that the sesquioxide of cobalt, a body which is so unstable that it does not form a single definite combination with acids, by union with ammonia should form a series of powerful bases. No more striking instances of the class of bodies which I have termed *conjugate* could have been furnished.

In the process of conjugation, it will be seen that an inactive body, brought into contact with an active one, becomes itself active and polar, just as a bar of soft iron brought into contact with a magnet becomes itself magnetic, and remains so, so long as the contact is unbroken. I think that there is more in this than a mere superficial analogy.

No theoretical views have done so much to make Organic Chemistry a science, as the discovery of the principle of homologism by Gerhardt. This principle must, for the present, be regarded simply as the expression of an empirical law, that series of bodies, between which there exists a constant difference of constitution, are analogous in chemical properties and relations. The discovery of wood-spirit and the investigation of its properties by Dumas, together with the subsequent discovery of fusel oil by Balard, first suggested the idea that alcohol is to be regarded as a generic, and not merely as a specific term. The same idea was soon applied to other organic compounds, and it gradually became evident that many well-known substances were only types of larger classes of similar bodies. It is, however, to Gerhardt that we owe the first clear perception and definite expression of the precise relation which exists between the chemical constitution of those organic substances which form natural families or groups, like the alcohols, ethers, &c. This relation consists simply in this, that bodies exhibiting a parallelism in chemical properties differ in constitution either by C_2H_2, or by some multiple of this expression. Bodies so related are said to be homologous, and the different members of such a series are called homologues. The elegance and simplicity with which whole classes of compounds can thus be brought under one general formula, and treated as a whole, is truly surprising. I will exemplify this remark by a few instances of homologous series, giving in the case of each the general formula which embraces every member as a particular case.

Hydrogens.	Acetenes.	Palenes.	Formic Acids.	Oleic Acids.	Oxalic Acids.
H	C_2H_2	H_2	$C_2H_2O_4$	$C_6H_4O_4$	C_2HO_4
C_2H_3	C_4H_4	C_2H_4	$C_4H_4O_4$	$C_8H_6O_4$	$C_4H_3O_4$
C_4H_5	C_6H_6	C_4H_6	$C_6H_6O_4$	$C_{10}H_8O_4$	$C_6H_5O_4$
C_6H_7	C_8H_8	C_6H_8	$C_8H_8O_4$	$C_{12}H_{10}O_4$	$C_8H_7O_4$
$C_{2n}H_{2n+1}$	$C_{2n}H_{2n}$	$C_{2n}H_{2n+2}$	$C_{2n}H_{2n}O_4$	$C_{2n}H_{2n-2}O_4$	$C_{2n}H_{2n-1}O_4$

To each of these general formulas the idea of certain specific properties is attached. Thus it is sufficient to know the general properties and relations of any one member of an homologous series to know those of all the others, which must, of necessity, correspond in kind, though not in degree. Stearic acid, $C_{34}H_{34}O_4$, and acetic acid, $C_4H_4O_4$, belong to the formic acid series. They differ greatly in appearance and in the ordinary physical properties, but chemically there is a a perfect parallelism between them. Of the possible upper limit in the case of each series, we, of course, know nothing; it is probable, however, that in each case such a limit actually exists. It appears, furthermore, desirable to extend the idea of homologism to other cases than where there is merely a difference of constitution of C_2H_2, by seeking for general analogies in properties wherever there is any regular and uniform difference of composition whatever. Thus the Phenyl series differs from the Ethyl series by eight equivalents of carbon which the former contains more than the latter, yet there are very strong points of analogy between Phenyl alcohol, $C_{12}H_6O_2$, and common alcohol, $C_4H_6O_2$. We have here a new species of homologism, the homologizing body being C_8. Kopp has shown that there exists between the boiling points of the successive homologous bodies of any series a common difference of about 19° C., and this remarkable relation, to which we shall presently return, has been of very great use in determining the true equivalents of many substances, and their position in the series to which they belong.

The theory of substitutions, as is well known, was proposed by Dumas in 1839, and the views of this distinguished chemist were based upon the observation that, in acetic acid, three equivalents of hydrogen could be replaced by three of chlorine, in such a manner that the new substance still possessed strongly acid properties, and, in many particulars, closely resembled ordinary acetic acid. It was hence inferred that chlorine could replace hydrogen, equivalent for equivalent, the properties of the original substance remaining essentially the same in all its derivatives.

It is of course unnecessary in this place to give a history of the controversy which ensued, and in which the most eminent chemists of the day took an active part. While, on the one hand, Berzelius and the electro-chemists maintained that a highly electro-negative body like chlorine could by no means play the part of an electro-positive element like hydrogen, the French chemists, on the other hand, replied by an imposing array of facts which admitted of no simple or satisfactory solution, except upon the theory of a double function of chlorine and similar elements. The question remained, however, undecided, and the views of both parties became more and more modified, until the discovery of the chlorinated alkaloids derived from anilin by Dr. Hofmann at last decided in favor of the modified theory of substitutions. This chemist proved, not merely that one or two equivalents of hydrogen in anilin could be replaced by one or two equivalents of chlorine or bromine, without a loss of basic properties, but also that the bases thus obtained could be reconverted into ordinary anilin. Melsens proved, in like manner, that chloracetic acid could be reconverted into common acetic acid. Thus it was shown that chlorine could replace hydrogen, and hydrogen be substituted for chlorine. In this case, then, it was clearly proved that both parties were right, and both wrong. The French chemists were wrong in asserting that the molecular structure and arrangement of the organic body alone determine its properties, since it has been clearly shown that, by the replacement even of a single atom of hydrogen by an atom of chlorine, the properties of the primitive undergo a more or less distinct modification. Thus chloranilin is less strongly basic than anilin, and trichloracetic acid a more powerful acid than the acetic.

The results of the investigations of the last few years upon the subject of substitutions may, I think, be summed up in the following propositions:—

1. One or more equivalents of hydrogen in a given organic compound may be replaced by an equal number of equivalents of chlorine, bromine, &c., without an essential change of properties.

2. In all cases, the chemical properties are changed in degree, if not in kind.

3. The more complex the constitution of an organic body, the less marked is the effect produced by the substitution of an equivalent of one element by an equivalent of another.

4. Conversely, the more simple the molecular structure of the primitive, the greater is the change in properties produced by an equivalent substitution.

5. The greater the number of equivalents replaced, the greater will be the change in chemical properties. In many cases, this change becomes so marked, that the properties of the original body are entirely lost, and only its molecular type remains.

6. The greater the difference between the chemical properties of the replaced and replacing substances, the greater will be the change in chemical character produced by the substitution. Hence electro-positive or zincous bodies replace each other, as we have seen in the case of Hofmann's ammonias, without modifying in any essential particular the properties of the primitive substance. And the same must be true for electro-negative or chlorous bodies, though their mutual replacements have hitherto been little studied.

From all these facts, it is clear that the chemical nature of a compound depends upon the *nature* of its constituents, and not upon their *arrangement* alone, as has been maintained.

If we adopt Ampère's theory of the binary structure of the elements, it appears to me that we can easily account for the replacement of an electro-positive body like hydrogen by an electro-negative one like chlorine. For of two associated molecules of chlorine, one will be positive and the other negative. Suppose that we are to replace one equivalent of hydrogen in anilin, $C_{12}H_7N$, by an equivalent of chlorine. We may assume that, in the presence of chlorine, the molecule of anilin becomes polar, an equivalent of hydrogen being positive, while the other elements, taken together, form the negative molecule. Then the reaction which ensues may be represented by the equation

$$\overset{-}{C_{12}H_6N}.\overset{+}{H} + \overset{-}{Cl}\overset{+}{Cl} = \overset{-}{C_{12}H_6N}.\overset{+}{Cl} + \overset{+}{H}\overset{-}{Cl}.$$

In this case it is clear that it is an electro-positive molecule of chlorine which replaces the more electro-positive hydrogen, and it is not difficult to conceive that the chlorine so introduced into the organic body may retain its electro-positive character. Of course, even the most highly electro-positive chlorine which we can produce must be electro-negative with respect to hydrogen, and this will explain the change in chemical character which its introduction produces. I offer these views with diffidence; they form part of a general theory of chemical polarity upon which I am engaged.

It is still a matter of dispute with chemists whether chlorine can replace oxygen, equivalent for equivalent. I consider the numerous class of bodies termed oxy-chlorides as furnishing conclusive evidence upon this point, and will cite, among organic compounds, the bodies whose formulas are as follows: —

Primitives	$C_4H_3O_3 + C_4H_3O_3$.	$C_4H_3O_3 + C_2HO_3$.	$C_4H_3O_3$.
Derivatives	$C_4H_3O_3 + C_4H_3OCl_2$.	$C_4H_3O_3 + C_2HOCl_2$.	$C_4H_3O.S.Cl$.

It is not doubted, I believe, by any chemist, that sulphur and oxygen freely replace each other in combination. The fact, as I shall show, is an important one.

To conclude this report according to the plan which I have laid down, it remains to offer a sketch of the present condition of Organic Chemistry. We have seen that the state of our knowledge compels the adoption both of the theory of compound radicals and of that of substitutions. There is, I think, but little difference of opinion between chemists upon these two points. The law of homology is universally admitted, while upon the theory of conjugate bodies there is at present but little discussion, our knowledge of the subject being still very imperfect. The great question of the day, as it seems to me, is a question of equivalents. What are the true equivalents of oxygen and its congeners, of chlorine and the chloroid bodies, of hydrogen and the metals, and, finally, of carbon and its compounds? Are we to adapt our combining weights to the combining volumes, so that the equivalents of all bodies shall be represented by the same volume, or shall we retain the equivalents now received, and admit that the combining volumes in the gaseous state are different? Lastly, are we to be content with the empirical formulas of compound substances, or may we hope to penetrate into their molecular structure so as finally to obtain their true rational constitution? These questions have all originated in the remarkable observations of Laurent and Gerhardt upon the number of equivalents which enter into the constitution of organic compounds. These chemists, by a careful examination of all known and well-determined formulas, have, I believe, indisputably proved that — with the single exception of carbonic oxide — in every organic compound not possessing acid properties, and not containing one of a certain class of acids to which I shall presently allude, the number of equivalents of carbon ($C = 6$) is even. For this there is certainly a reason. Gerhardt and Laurent assume that the equivalent of carbon is 12, and not 6, as heretofore maintained, and that consequently in all our formulas the number of equivalents of this element must be reduced one half; and at first sight this appears to be a perfectly reasonable assumption. On the other hand, however, in a very large number of organic acids, — carbonic acid being one, — the number of equivalents of carbon is odd, if the acids in question be considered as unibasic. Gerhardt and Laurent double the equivalents of these acids, and regard them as bibasic; in this manner the new equivalent of carbon, viz. twelve, satisfies all the formulas. As a proof of the correctness of this view, the French chemists show that all of these organic acids possess distinct but common properties, which in any view oblige us to place them in a class by themselves. Why not, then, assume the totality of these common properties as the definition of a bibasic acid? But admit this definition, and many inorganic acids, as, for example, sulphurous and sulphuric acids, become bibasic, and here we soon meet with serious difficulties. But, furthermore, Gerhardt and Laurent show that, in very many organic compounds, the number of equivalents of oxygen is also even. They therefore double the equivalent of this element, and are consequently obliged to double the equivalents of all those bodies, inorganic as well as organic, in which the number of equivalents of oxygen, as they are now written, is odd. In this manner the formula of water becomes $O'H_2$, ether is $C'_4H_{10}O'$, and anhydrous acetic acid $C'_4H_6O'_3$, where the accent is used to distinguish the new equivalent. It will be immediately seen that, if the equivalent of oxygen be doubled, a radical change must be made in all the formulas of inorganic chemistry into which oxygen enters. Thus, all protoxides must be written R_2O'; the deutoxides become RO'; the nitrates become NO'_3R, and the nitrites NO'_2R. The supposition of the French chemists, that the volumes of all bodies in the gaseous state are equal, leads also at once to the conclusion, that the equivalents of oxygen and carbon are respectively sixteen and twelve ($H = 1$). Finally, a careful study of the relations between the physical properties of organic compounds *appears* to afford a complete confirmation of the new views. Thus, Kopp long since showed that between the boiling points of the successive members of any homologous series there exists a common difference of about 19° C. In a recent discussion of the formulas of the alcohols, ethers, and anhydrous organic acids, Will has shown that, if Kopp's law be admitted, the formulas of the ethers and anhydrous acids must be doubled, as the double equivalent of oxygen requires. Again, Kopp has proved that, in order to obtain the true specific volumes of organic compounds, the densities of the bodies must be taken at or near their boiling points. By *specific volumes* it will be remembered that we understand the relative spaces occupied by the equivalents, and these are calculated by dividing the equivalents by the corresponding densities. Now, a comparison of the specific volumes of fluid compounds appears to show that the equivalents of ether and the anhydrous acids above mentioned are twice as high as usually admitted. The boiling points, as well as the specific volumes, of the fluid compounds hitherto examined, may be brought under one point of view by assuming, with Williamson and Gerhardt, that the formula of water is $O'H_2$, and that the unibasic acids, ethers, alcohols, and anhydrous acids may all be considered as derived from water by the replacement of one or both the equivalents of hydrogen by other radicals. The following formulas will sufficiently illustrate these points:—

Water.	Alcohol.	Ether.	Acetic Acid.
$O'\left\{\begin{matrix}H\\H\end{matrix}\right.$	$O'\left\{\begin{matrix}H\\C_4H_5\end{matrix}\right.$	$O'\left\{\begin{matrix}C_4H_5\\C_4H_5\end{matrix}\right.$	$O'\left\{\begin{matrix}C_4H_3O'\\H\end{matrix}\right.$

Anh. Ac. Acid.	Mixed Ether.	Mix. Acid.
$O'\left\{\begin{matrix}C_4H_3O'\\C_4H_2O'\end{matrix}\right.$	$O'\left\{\begin{matrix}C_2H_3\\C_4H_5\end{matrix}\right.$	$O'\left\{\begin{matrix}C_4H_3O'\\C_2HO'\end{matrix}\right.$.

Purely physical considerations certainly, then, give us every reason to hope that we may hereafter determine the true molecular structure of organic bodies, and that our knowledge is not to be limited to empirical formulas.

It must then, I think, be admitted that the evidence in favor of doubling the received equivalents of carbon and oxygen is of great weight, and that the adoption of this change will greatly simplify most of our formulas. Let us then briefly consider the objections to these changes. In the first place, I remark, that if the equivalent of oxygen be doubled, that of sulphur must also be changed in the same ratio. Now, the volume of the vapor of sulphur is one third of that of oxygen. If we admit, with the French chemists, that all bodies have the same volume in the gaseous state, the equivalent of sulphur as now received must be multiplied, not by 2, but by 6. There will then be no analogy between the formulas of the compounds of sulphur and those of oxygen, between which the chemical parallelism is perfect. Again, the researches of Frankland have recently shown that the volume of gaseous zinc is not two, like that of hydrogen, which the French chemists consider as the type of all the metals, but one, like that of oxygen and carbon. And, generally, experiment shows that elements which belong to the same natural group do not always possess the same volume in the state of vapor. It becomes, then, from this point of view, very difficult to admit the theory of equal volumes and the equivalents which flow from it. In the second place, it appears, I think, necessary to admit that chlorine with the equivalent 35.5 = (2 vols.) can replace oxygen with the equivalent 8 = (1 vol.), and it is certain that sulphur eq. 16 = ($\frac{1}{3}$ vol.) can replace oxygen, the combining volume of which is three times as high. The third obvious difficulty arises from the constitution of the salts of the sesquioxides. Thus alum is $KO, SO_3 + Al_2O_3, 3\,SO_3 + 24\,HO$, with the usually received equivalents. But if sulphuric acid be bibasic, how are we to explain such compounds as $Al_2O_3, 3\,SO_3$? Gerhardt very ingeniously supposes that in the so-called *sesquioxides* there exist metals with two thirds of the equivalents of the same metals in their *protoxides*. Thus sesquioxide of iron is not Fe_2O_3, but 3 feO, where fe = $\frac{2}{3}$ Fe. This view leads at once to many very simple and beautiful formulas, but it leaves unexplained the fact that the new protoxide feO and its congeners so frequently enter into combination in the proportion of three equivalents. Lastly, we may remark that the indications afforded by a comparison of physical constants are at best somewhat precarious and uncertain. Thus may it not be that, in the case of Kopp's law of the difference between the boiling points of homologous bodies, the true expression should be, "Homologous bodies differ in boiling points either by 19° C., or by some multiple of this number"? Again, while the boiling points of the oxygen compounds of ethyl, acetyl, &c. appear to confirm the views of Laurent and Gerhardt, those of the corresponding sulphur compounds appear to follow entirely different laws. Thus the sulphides of ethyl and methyl are less volatile than sulphide of hydrogen, while the oxides of the same radicals are more volatile than water. The oxygen alcohols of ethyl and methyl are less volatile than their ethers, the sulphur alcohols are more volatile than the corresponding sulphides. If the existence of a certain law, connecting the boiling points of a series of oxides, proves that these oxides have a particular molecular constitution, then the absence of such a law, or the existence of a converse one, proves that the corresponding sulphides have a different constitution. It is to be hoped that the very interesting and valuable researches of Kopp on the boiling points and specific volumes of organic bodies will be extended to those which contain sulphur. These then are the questions upon which the opinions of chemists are now divided. They amount to presenting us with this alternative,— Shall we extend the laws which govern the constitution of inorganic bodies to organic compounds, or shall organic chemistry embrace inorganic chemistry as a particular case?

In concluding a report which, I am conscious, presents but a superficial sketch of its subject, I have only to ask that the nature of the occasion, and the point of view from which I have been compelled to write, may be duly considered.

PROCEEDINGS

OF THE

PROVIDENCE MEETING, 1855.

COMMUNICATIONS.

A. MATHEMATICS AND PHYSICS.

I. MATHEMATICS.

1. On Solutions by Construction. By Rev. Thomas Hill, of Waltham, Mass.

The refined analysis of modern days, aided by the invention of logarithms and other facilities for computation, has almost entirely banished solutions by construction from the office of the engineer. It has also done a more serious mischief, by almost entirely banishing Geometry from the course of education.

I need not attempt to show that this must be a mischievous error, for every student of the mathematical sciences knows that Geometry must ever be the foundation of all learning. I shall confine myself to the other point, — the value to the practical mathematician of solutions by construction. My attention has been called anew to this subject by a memoir of Professor Cooke in the Memoirs of the American Academy, in which he illustrates a paper upon the compounds of zinc and antimony by a curve line, possessing singular properties, which could not have been brought to light if the curve had not been first made visible to the eye. When a function is geometrically constructed, or a series of numbers rendered visible by being represented by lines, the continuity or discontinuity of the curve is much more readily perceived than it can be from an inspection of the numbers themselves.

It is true that geometrical construction can seldom be accurate in the fourth decimal place, counting from the highest place mapped, and therefore, when greater accuracy than this is required, calculation is indispensable. But in very many of the practical cases which actually occur, no greater accuracy is desired, or is even possible; and in such cases we should avail ourselves of geometrical construction, if that construction will give us the result more rapidly.

Interpolation

Affords us a good example. In interpolating among a series of observed numbers, some assumption as to the nature of the curve must always be made, and hence perfect accuracy of results is impossible. We may therefore generally obtain equally good results by construction, and in much less time than would be required to calculate them.

If there are three observed points, a circle passing through them is the most natural curve. If there are more than three, we may connect them by a succession of chords, and the mutual intersection of perpendiculars raised upon the centres of these chords will make a part of a polygon tangent to a curve, whose involute will be a curve passing through the given points. By drawing this evolute by the eye, and setting in a few pins, we can, with a string, strike the required curve.

In this way we can almost instantly make any required number of interpolations, accurate to the hundredth of an inch, which, on the usual scale of units in mapping, is as accurate as the observations require; that is, it is within hundredths of the difference between two successive observations. If greater accuracy is required, recourse must be had to calculation, by Alexander's formula in Silliman's Journal for January, 1849, or some other usual formula.

That formula and my own mode by projecting the evolute both give curves which exactly satisfy all the observations. But there are frequently cases in which we know that the observations ought not to be exactly satisfied. We may, for instance, know that the curve ought to be a parabola or ellipse, and yet find that the curve through the given points has contrary flexures. In this case the alternate observations may be connected, and two evolutes found. An evolute may then be drawn by the eye equidistant between them, and the involute drawn with a string long enough to make it pass equidistantly among the observed points. In case of sufficient observations to allow the two curves to be well determined, this plan is very successful. In the case of a small number of points, there is nothing left but for the eye to determine what weight shall be allowed to each observation, an operation which, in ordinary cases, may be as safely trusted to the eye as to the pencil. The "fudging" of practical engineers, and the rule of Double Position, stand on the same foundation. They should not be frowned upon as inexact modes of discovering truth. Their method lies at the basis not only of all science, but of politics, agriculture, commerce, medicine, jurisprudence, and I almost might say morality.

Rejection of Doubtful Observations.

When observations are mapped for the purpose of interpolation, or for other purposes, the eye will instantly detect the observations to be rejected. In most practical cases, excepting in the marvellously minute and accurate calculations of astronomy, the application of Peirce's Criterion would consume time disproportionate in value to the result. In all such cases, a glance of the eye at the observations, when mapped or plotted, will tell which observation ought to be rejected, and the mapping of the observations for such a purpose need not be at all carefully done. Where the eye hesitates, the formula would also hesitate; that is, it would require the numbers to be carried out to a higher decimal place.

Solutions of Problems.

In the solutions of problems by geometrical construction, we are to be guided always by two considerations, — the degree of accuracy desired, and the relative rapidity of construction and computation. For ordinary calculations of trigonometry, nothing would be gained in time by geometry, and much lost in accuracy. But there are other problems of more intricacy, and in which great accuracy is not required, in which a mechanical construction gives results very rapidly. In eclipses of the sun, for example, it seems a great waste of labor to calculate with accuracy the phases for the whole continent, when the day of the eclipse may be signalized by one of those extended north-east storms that cloud half the continent with an impenetrable veil. To save this waste of labor, I was requested some years ago by Professor Peirce to invent a machine to perform these calculations. By this machine I computed more than two hundred phases of the eclipse of May, 1854, in ten hours' time, and found that most of my places agreed to the nearest minute with the accurate map afterwards issued for the Nautical Almanac Office. Last Friday I spent about four hours in calculating the chart, accompanying this paper, of the eclipse of March, 1857.*

But I have said enough to illustrate the central thought of this paper. I wished to show that there are many cases in which the scientific computer could save time, without the sacrifice of any desirable degree of accuracy, by appealing to his eye and compasses to do the work of the pen; and many others in which, like Professor Cooke, in the memoir to which I have alluded, he could obtain much clearer ideas of the nature of a function by looking at the observations of its phenomena drawn as a curve, instead of being arranged in tabular numbers. The time of a good computer is certainly too valuable to be wasted in labors upon rough data, to which construction is better adapted; or in revising intricate and tedious calculations, when geometrical tests can be applied which will decide the question at a glance.

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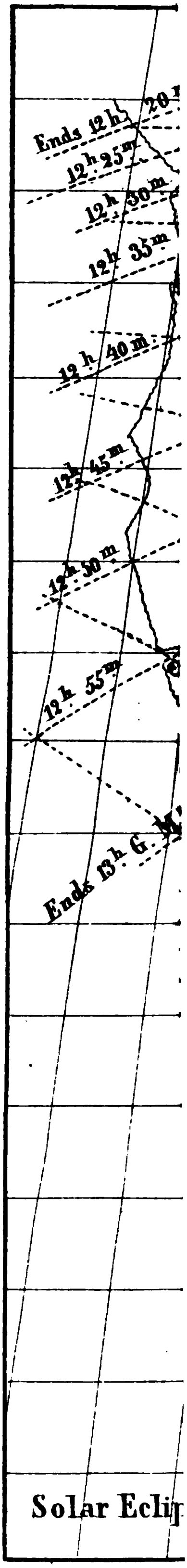
* The chart has had some additional labor since bestowed upon it, but not more than six hours were necessary from the time the Tables of the Moon were first opened, until the chart was finished in its present form.



Ends 12h
20
12h 25m
12h 30m
12h 35m
12h 40m
12h 45m
12h 50m
12h 55m
Ends 13h G. M.
Solar Eclip

2. Abstract of a Paper on Researches in Analytic Morphology. Transformation of Curves. By Professor Benjamin Peirce, of Cambridge.

The singular changes which the forms of curves undergo by variations of the elements upon which they depend, deserve philosophical study, and may serve to illustrate the varieties of form which are observed in the organic world to originate from the different fundamental types. The following are the special points which are the subject of the present investigation.

1st. *The recurrence of the same forms in radically different types.* Thus the circle is one of the conic sections, it is an elastic curve, it is the extreme limit of the curves which are similar to their own involute, it belongs to the class of catenary upon any vertical surface of revolution, and on account of its simplicity must occur in almost every symmetrical system of curves. Its occurrence in each of these classes is not, therefore, a valid argument against a system of classification in which any or all of these types should be recognized as separate groups, nor is it a proof that such a system is artificial and opposed to sound logic. But, on the contrary, it would seem to indicate that, as in the forms which might originate in our intellect there would be a true crossing and interlacing of thought, the same thing may be reasonably expected in the forms which proceed from the Divine intellect, so that in a true system of nature the very same type would be found in quite different portions of the classification. In the same way the equilateral hyperbola which is associated with the ellipse as a conic section, is wholly separated from this curve, and even from all the other hyperbolas when it becomes a catenary upon the vertical cone. Geometry is rich in similar instances of this recurrence of the same form in different types.

2d. *The great variety of apparently different forms which are subject to the same intellectual law of development.* The conic sections are familiar examples of this phenomenon, and the elastic curve, which begins in the straight line, is slightly undulatory in its first curvation, then bends back upon itself in large folds which please the eye by their regular intersections, then is compressed into the simple form of a figure *eight* (8), by still further pressure assumes another variety of intersecting folds, becomes then a system of delicate loops which twist alternately in opposite directions, and are continually removed from each other until they are reduced to a single loop with two infinite branches which affect at their extremities a form of especial simplicity. The elastic curve next assumes a form of loops in which the curvation is always in the same direction, and these loops approach by increased compression until they intersect each other, and are finally reduced to the single form of the circle which may be called the elastic ring.

3d. *The different appearance of the same intellectual idea when clothed in a different material form.* Thus the variety of form of the elastic curve disappears in the motion of the pendulum, which is the expression of the same law of thought, but which only exhibits the monotony of perpetual vibration, or the equal monotony of continual rotation in the same direction with periodic returns of the same velocity of motion, when it is urged with sufficient rapidity to make a complete revolution. The conceptions of the ellipse and hyperbola with reference to the distance of their points from their foci seem to be fitly expressed in the difference of their forms; but when these same conceptions are transferred to the surface of the sphere, the forms become identical, and the conceptions are to be regarded as slightly different and mutually dependent properties of the same curve.

4th. *The changes in the mutual relation of the parts of the curve which accompany its changes of form.* This is particularly conspicuous when the curve passes through any of its singular states. Thus when the axes of the hyperbola vanish, the curve becomes two straight lines which cut each other, but one half of each line belongs to one of the branches of the original hyperbola, while the other half belongs to the other branch. These same two lines are also the evanescent state of a different hyperbola, of which the branches are situated in the other angles of the lines. If the lines are assumed to represent the first hyperbola, the halves which represent a branch by their combination are different from those which belong together in the second hyperbola; so that, upon reaching the point of intersection by moving towards it along one of the lines, it is uncertain always on which half of the other line the motion should be properly continued. It is interesting to observe that, on the occurrence of such cases in nature, the decision is always avoided by some simultaneous phenomenon which renders it unnecessary. Thus, in the rotation of a free solid, when the body begins to rotate about an axis of rotation which is upon the line of direct approach to the mean axis of rotation, it would be doubtful, when the axis of rotation reached the mean axis, in which direction it should begin to move away from this position; but this uncertainty is wholly speculative, for the velocity of approach towards the mean axis constantly diminishes until it is infinitely small in the immediate vicinity of this axis, so that the decision of the question must be postponed till the close of an infinite time.

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3. Abstract of a Paper upon the Solution of the Adams Prize Problem for 1857. By Professor Benjamin Peirce, of Cambridge.

The following announcement of the Adams Prize is from the "Quarterly Journal of Pure and Applied Mathematics," edited by Professor Sylvester and Mr. Ferrers, assisted by Professor Stokes, Mr. Cayley, and M. Hermite: —

"The University of Cambridge having accepted a fund raised by several members of St. John's College, for the purpose of founding a Prize, to be called the Adams Prize, for the best essay on some subject of pure mathematics, astronomy, or other branch of natural philosophy, the prize to be given once in two years, and to be open to the competition of all persons who have at any time been admitted to a degree in this University, the examiners have given notice that the following is the subject for the prize to be adjudged in 1857: —

"*The Motion of Saturn's Rings.*

"The problem may be treated on the supposition that the system of rings is exactly or very approximately concentric with Saturn, and symmetrically disposed about the plane of his equator, and different hypotheses may be made respecting the physical constitution of the rings. It may be supposed, (1.) that they are rigid; (2.) that they are fluid, or in part aeriform; (3.) that they consist of masses of matter not mutually coherent. The question will be considered to be answered by ascertaining, on these hypotheses severally, whether the conditions of mechanical stability are satisfied by the mutual attractions and motions of the planet and the rings.

"It is desirable that an attempt should also be made to determine on which of the above hypotheses the appearances both of the bright rings and the recently discovered dark ring may be most satisfactorily explained; and to indicate any cause to which a change of form, such as is supposed, from a comparison of modern with the earliest observations, to have taken place, may be attributed.

"The essay must be sent in to the Vice-Chancellor on or before the 16th of October, 1856, privately; each is to have some motto prefixed, and to be accompanied by a paper sealed up, with the same motto outside, which paper is to inclose another, folded up, having the candidate's name and college written within.

"The papers containing the names of those candidates who may not succeed, will be destroyed unopened.

"Any candidate is at liberty to send in his essay printed or lithographed.

"The successful candidate will receive £130. He is required to print his essay at his own expense, and to present a copy to the University Library, to the Library of St. John's College, and to each of the four Examiners."

Prizes of this kind have not yet been established in America, and it is to be hoped that they never will be; for they serve to divert an excess of intellectual power from its natural channel, and concentrate it upon a single object of research, obtaining thereby many solutions of a problem, of which only one can be required. They are peculiarly unsuited to an atmosphere of free thought, and can only receive the cordial approbation and response of minds which are hardened to the trammels of despotism or oligarchy. The only prizes which can be regarded as of unalloyed utility are those which, like the Rumford Premium, the medals of the Royal Society, the Cuvier Prize, and many others connected with the science of France and Germany, are awarded to successful labors actually performed under the unrestricted influence of genuine philosophic inspiration. The present case deserves especial criticism, in that a prize is not likely to elevate the science of a community from which foreign competition is carefully excluded.

The present subject of the Prize has been already discussed before this Association; and in a memoir, which was read at the Cincinnati meeting, the fundamental problem was solved in a most general form, and the principles of the solution have been published in Gould's Astronomical Journal, with sufficient detail to enable any geometer of high ability to supply the deficient formulæ. Other pursuits have prevented the final working of the formulæ into forms of sufficient elegance for publication. The restrictions of the present prize greatly facilitate the discussion, and enable the mathematical development to assume such a simplicity that it may easily come within the grasp of ordinary geometrical capacity, and I have therefore thought it expedient to present it, in this form, to the Association, and it will be immediately published in Gould's Journal. The only important change from the general mode of discussion consists in the application of the theory of the Potential, given by Gauss, and still further developed in a treatise of Analytic Mechanics which is soon to be published. The argument against the solidity of the ring will probably be admitted to be satisfactory and complete. The objections to a ring composed of discontinuous materials are derived from the internal currents to which such a system must be liable, and which cannot fail to reduce it either to a powder or to a fluid state; so that no other reasonable hypothesis remains but that of a fluid ring.

The various changes in the constitution of the ring, which have been observed by astronomers, are confirmatory evidence of its fluid nature. The appearance and disappearance of the finer divisions indicate internal commotions and collisions, which are likely to be associated with a loss of living force. The decrease of the internal diameter of the ring, which is indicated by the able researches of Otto Struve, with an accumulation of testimony which is hard to be resisted, may perhaps be the effect, and therefore a manifestation, of this loss of power; in which case, it would seem that a portion of the ring is likely to fall upon the primary before another century has elapsed. Or this phenomenon may be a great secular tide produced by the action of the satellites, which, however, could hardly occur without a sensible loss of force; so that even in this case an important change in the constitution of the ring would not seem to be a very remote contingency. But the action of the satellites upon the ring is not included in the demands of the prize, so that the discussion of the tides of the ring will be postponed to a subsequent occasion.

4. Abstract of a Paper on Partial Multipliers of Differential Equations. By Professor Benjamin Peirce, of Cambridge.

Two systems of multipliers of simultaneous differential equations have been made the subject of previous investigation, one of which was discovered by Euler, and the other by Jacobi. The contrast between these multipliers is curious and interesting; for every system of Eulerian multipliers corresponds to a first integral of the given equations, while the Jacobian multiplier corresponds to a last integral of the equations. In comparing and examining these multipliers, I have been led to the discovery of a general system of multipliers, which includes both of those previously known. They are such that, for every system of these multipliers which is of any order whatever, there is a corresponding integral of the given equations. The mathematical definition and investigation of these multipliers will be published in Gould's Astronomical Journal and in the System of Analytic Mechanics.

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5. Abstract of a Paper upon the Catenary on the Vertical Right Cone. By Professor Benjamin Peirce, of Cambridge.

The investigation of the form of a uniform chain upon a surface of revolution, of which the axis is vertical, may be referred to that of the *asymptotic equilateral hyperboloid*, which has the same axis; that is, to the surface formed by the revolution of an equilateral hyperbola about either of its asymptotes. The catenary upon this hyperboloid crosses the meridian curve at a constant angle; and if the centre of the hyperboloid is rightly placed upon the axis of the given surface, the angles which the curve makes with the meridian of the surface will be the same as all the circles of its intersection with the hyperboloid. If, then, the hyperboloid is made of such a magnitude as to pass through one of the limiting points of the catenary, that is, one of the points in which the curve is horizontal, it must pass through all the other limiting points of the catenary. Hence the limits of the catenary are derived from the simple inspection of the points of intersection of the meridian curve of the surface of revolution, with a fitly adjusted equilateral hyperbola. In the case of the right cone, the meridian curve is a straight line, so that the determination of the points of the catenary upon a vertical right cone involves the discussion of the intersections of a straight line with an equilateral hyperbola.

The catenary extends, in all cases, over those portions of the surface of revolution which are more remote from the axis of revolution than those of the limiting hyperboloid which are upon the same level, and is excluded from the other portions of this surface. In the case of the vertical right cone, the catenary may consist of three portions, of which the intermediate portion may return into itself, while the other two portions extend to infinity; or the intermediate portion may wholly disappear. The complete definition of this catenary is effected by the aid of elliptic integrals.

The formulæ will be published in the Analytic Mechanics.

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6. Abstract of a Paper upon the Motion of a Heavy Body on the Circumference of a Circle which rotates uniformly about a Vertical Axis. By Professor Benjamin Peirce, of Cambridge.

The motion, in this problem, may be either oscillatory or continually in the same direction, with a certain law of periodicity. In the case of the motion continued in the same direction, there are usually two points of maximum and two of minimum velocity, which are determined by the intersections of the circumference with an equilateral hyperbola which is drawn in its plane so as to pass through the centre of the circle, with one of its asymptotes horizontal, and of which the co-ordinates of the centre are quite simply determined. When the motion is oscillatory, the two points of maximum velocity may remain, but one of the points of minimum velocity must be on the portion of the circumference which the body does not pass through.

The case of the coincidence of one of the points of maximum with one of those of minimum velocity, leads to a new and simple geometrical property of the equilateral hyperbola.

The case in which the initial velocity of the body is just sufficient to carry it to one of the points of minimum velocity, admits of complete integration.

The full investigation of this problem is contained in the Analytic Mechanics.

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7. Abstract of a Paper on the Resistance to the Motion of the Pendulum. By Professor Benjamin Peirce, of Cambridge.

This paper was devoted to the examination and discussion of the experiments of Newton, Dubuat, Borda, Bessel, and Baily upon the vibrations of pendulums, and much doubt was thrown upon the correctness of the generally received opinion, that the resistance to the motion of a very delicately mounted pendulum is exclusively attributable to the atmosphere. It was suggested that a sensible part of the motion which was thought to be lost from this cause, might be carried off by molecular vibration through the point of support, and this subject seems to deserve careful examination by well-conducted experiments.

The details of this communication will be published in the Analytic Mechanics.

II. ASTRONOMY.

1. On the Temperature of the Planets, and on some of the Conclusions resulting from this Temperature. By Professor Elias Loomis, of New York.

I propose to inquire into the probable temperature of the different bodies which compose our solar system, and shall briefly advert to a few of the conclusions resulting from this temperature. I am aware of the uncertain nature of many of the data involved in this inquiry, and that there is room for some diversity of opinion respecting my conclusions; but if this communication shall have the effect to stimulate others to an investigation of the same subject, my labor will not be without its reward.

I shall commence my examination with that body with which we are best acquainted, and shall inquire what would be the temperature of our globe if the heat of the sun were withdrawn; or what would be the temperature indicated by a thermometer placed where the earth now is, supposing the bodies which compose the solar system to be annihilated. It is evident that the planetary spaces have a certain constant temperature, which is independent of the presence of the sun, and of the original heat which the earth and planets have preserved; for, otherwise, the polar regions of the earth would be subject to an intense cold, and the decrease of temperature from the equator to the poles would be far more rapid than what we actually observe. This constant temperature of space is necessarily lower than the mean temperature of the coldest regions of the earth; but perhaps not lower than those occasional extremes which may result from evaporation and an extraordinary expansion of air. The mean temperature of the month of January at Jakutzh, Siberia, in Lat. 62°, according to Professor Dove, is 45° below zero of Fahrenheit; and the thermometer at that place has been observed to sink to 76° below zero. We must conclude that the former temperature was somewhat influenced by the action of the sun, for the polar regions during the winter do not entirely lose the effect received from the sun's rays during the preceding summer; and through the intervention of winds, there is a continued interchange between the temperature of the polar and the equatorial regions.

The temperature of the planetary spaces has been variously estimated by different philosophers. Poisson estimated it at 8° above zero of Fahrenheit; but how it is possible that the temperature which our globe would take if the sun did not heat it at all, should be higher than the mean temperature of many points of its surface exposed to the solar rays for more than six months of the year, is a paradox which Poisson himself could not explain. Valz estimated the temperature of the planetary spaces at 49° below zero; Fourier at 58°; Arago, 70°; Ivory, 74°; Peclet, 76°; Laplace, 91°; Saigey, 96°; Sir J. Herschel, 132°; and Pouillet estimated it as at least 175° below zero. The mean of all these estimates is 81° below zero, which I adopt as a tolerable approximation to the temperature of the planetary spaces. In forming this mean, I allow to each of the estimates an equal weight, because I think it probable that the estimate of Pouillet errs as much on one extreme as that of Poisson does on the other.

The temperature of the earth's equator is about 82°. But were it not for the influence of the sun, this temperature would be 81° below zero; for the internal heat of the earth, however great it may be at considerable depths, is insensible at the surface. The effect of the sun's rays is therefore to elevate the mean temperature of the equator 163°, and the mean temperature of the poles about 70°.

If now we assume that the intensity of the sun's heat varies inversely as the square of the distance, we can compute the mean temperature which the earth would have if situated at the distance of either of the other planets from the sun. We shall find that, at the distance of Uranus or Neptune, the temperature of its equator would differ less than one degree from that of the celestial spaces; at the distance of Saturn, it would differ less than two degrees; and at the distance of Jupiter, the temperature of its equator would be only six degrees above that of the celestial spaces. Each of these planets is much larger than the earth, and for that reason would be longer in losing its primitive heat; but as soon as a solid crust of a few miles in depth was formed, it is probable that the temperature of the surface would be but slightly influenced by the internal heat. In other words, we conclude that the temperature of the surface of Jupiter, and of all the more remote planets, is sensibly the same as that of the celestial spaces, or 81° below zero of Fahrenheit.

This conclusion will not be materially affected by supposing these planets to have a much greater power of absorbing the sun's heat than the earth; for the same physical conditions which increase the absorbing power increase also the radiating power; so that although a planet might absorb more heat during the day, it would lose more heat during the night by radiation.

An atmosphere tends, by its mobility, to equalize the effect of the sun's action; and it also tends to increase the mean temperature of the planet. The heat which comes directly from the sun penetrates the air more readily than the heat which proceeds from non-luminous sources. Were it not for the mobility of the air, the heat of the sun would therefore accumulate in the lower strata of the atmosphere, which would thus acquire a very high temperature; but since the air rises as soon as it is heated, the radiation of its heat is thereby promoted; and the effects which would take place in a transparent and solid atmosphere are greatly diminished, though not entirely neutralized in their character. Making the most liberal allowance for the effect of an atmosphere, however dense, we cannot suppose the mean temperature of Jupiter's equator to be less than 70° below zero, and that of the remoter planets less than about 80° below zero.

If the earth were situated at the distance of Mars, the heat which its equatorial regions would receive from the sun would amount to 70°, corresponding to a mean temperature of 11° below zero at the equator, and of 51° below zero at the poles. There is nothing in the atmosphere of Mars to lead us to infer that these numbers differ much from the actual temperature of that planet.

If the earth were situated at the distance of Venus, the heat which its equatorial regions would receive from the sun would amount to 311°, corresponding to a temperature of 230° at the equator, and of 52° at the poles. As Venus is admitted to have a dense atmosphere, it is probable that its actual temperature is not below the preceding estimate. The mean temperature of Mercury's equator, computed in the same manner, is 1006°, and that of its poles 386°.

If the moon had an atmosphere of the same density as the earth, its mean temperature would be the same as that of the earth, since it has the same mean distance from the sun; but since it has no appreciable atmosphere, its mean temperature must be lowered. I think we shall not be far from the truth in assuming the mean temperature of the moon's equator at 40°; but this temperature must be subject to far greater variations than that of the earth, because there is no atmosphere with its clouds to protect the moon's surface from the full effect of the sun's rays during their long day, which is twenty-eight times the length of our day; and radiation is entirely unimpeded during their night, which is also twenty-eight times the length of our night.

Let us now inquire how far the temperatures assigned for the different planets are compatible with the existence of animal or vegetable life. We have found that the temperature of every part of the surface of Jupiter, as well as that of the more remote planets, is not far from 80° below zero. We may then conclude that no form of animal or vegetable life with which we are acquainted can exist upon either of these planets.

We have found upon Mars a mean temperature of 11° below zero at the equator, and of 51° below zero at the poles. On the summits of mountains, and in the polar regions of the earth, the snow sometimes assumes a red tinge, produced by particles of coloring matter less than the thousandth of an inch in diameter. These particles have been discovered to be of a vegetable character. A species of vegetation may therefore exist even at the temperature of a polar winter. But this vegetation is wholly microscopic, and, so far as we are acquainted, the temperature of Mars is incompatible with any form of animal or vegetable life exceeding the humblest dimensions.

The mean temperature of the equator of Venus is above that of boiling water, while at its poles we find a mean temperature of 52°. The polar regions of Venus have, therefore, a temperature adapted to the development of animal and vegetable life in their highest perfection, while at the equator no form of life with which we are acquainted could exist.

The temperature of the surface of Mercury, even at its poles, is too elevated for the support of any form of animal or vegetable life.

The existence of life upon the moon is impossible on account of the absence of an atmosphere. I am aware that Professor Hansen has expressed the opinion, that the centre of gravity of the moon does not coincide with its centre of figure, but is about thirty-six miles more distant from us; and hence, that between that hemisphere of the moon which is turned towards the earth, and that which is turned away from us, there must exist a considerable difference with respect to level, climate, &c. If we suppose the figure of the moon to be a sphere, (and observation has never indicated any inequality in its diameters,) then, according to Professor Hansen, the centre of the visible disc of the moon lies about thirty-six miles above the mean level of the moon, and the centre of the opposite hemisphere lies about as much below the same level. If the moon had an atmosphere of the same density as the earth, this atmosphere would be wholly withdrawn from the middle of the hemisphere which is turned towards the earth, as it would be from the summit of a mountain thirty-six miles in height; but on the opposite hemisphere the atmosphere would have a proportionally greater density. Professor Hansen admits that near the moon's limb we might reasonably expect to discover some trace of an atmosphere. But since no appreciable refraction has been observed in the case of any occultation of a star by the moon, we conclude that there is no sensible atmosphere at the moon's limb, and consequently there can be no dense atmosphere on the opposite hemisphere of the moon.

We have concluded the temperature of Saturn to be about 80° below zero. Some have inferred that Saturn's ring was a liquid body; but what substance could retain the liquid condition at so low a temperature? Not only water, and the more common liquids on the earth's surface, solidify at a much higher temperature, but so also do most of the acids, as nitric acid, sulphuric acid, &c. Indeed, pure alcohol is almost the only substance with which we are generally familiar, which can endure so low a temperature without solidifying. The specific gravity of alcohol does not differ much from the specific gravity of Saturn, as determined by astronomical observations.

It may be said that we know nothing of the composition of the liquids which may exist on the surface of Saturn, and that the Creator may have furnished that planet with a liquid covering suited to its condition, but entirely unlike anything with which we are acquainted. If, however, the solar system is *one system*, and was evolved from the elementary condition under the operation of general laws, as geologists are pretty well agreed in maintaining, then each of the planets is probably composed mainly of the same elementary substances; and since these elements can only combine in certain definite proportions, every substance existing on Saturn also exists, *potentially*, if not *actually*, in the laboratory of the terrestrial chemist. This conclusion is confirmed by an examination of Meteorites, which are believed to be bodies foreign to the earth, and which contain no elements not found in terrestrial bodies. If the progress of scientific discovery should confirm the conclusion that alcohol is the prevalent liquid on the surface of Saturn, then it may be proper to inquire, Whether the stomachs of animals may be so constituted that alcohol shall be an innocent beverage?

There is a different substance known to chemists, which some may conceive to constitute the prevalent liquid on Saturn. Common water is a compound formed by the union of one atom of oxygen with one atom of hydrogen. There is another compound formed by the union of two atoms of oxygen with one atom of hydrogen, and this is called the deutoxide of hydrogen. This substance remains fluid at every degree of cold which has been applied to it. It is, however, heavier than water, a circumstance which appears to decide that this substance is not the prevalent liquid on Saturn.

I will venture to suggest, whether all the peculiarities of Saturn's ring, including the apparently variable number and breadth of the divisions, as well as the recent formation of a new ring, may not be explained by supposing the ring to consist of an immense number of solid bodies, of small dimensions, not cohering together, each revolving independently about the primary, and forming in effect an immense number of independent moons. We know from the example of the nebulæ, that distinct points of light, when sufficiently near to each other, may produce the impression of a uniformly illumined surface. The mutual perturbations of these moons might easily change the number and breadth of the divisions of the rings, and the contracting of the orbits of some of the moons might form a new ring of variable dimensions; while its faint light and wonderful transparency would be explained by supposing the moons to be separated from each other by intervals greater than in the case of the old rings.

About the polar regions of Mars are observed circular spots of dazzling whiteness, which have suggested the idea of polar snows accumulated during the long winter, and which are partially dissolved during the protracted summer. We have concluded that the mean temperature of the equator of Mars is 11° below zero, and that of its poles 50° below zero. With such a temperature, terrestrial snow would never dissolve, and we must call upon the chemists to inform us what can be the composition of Martial snow which melts at a temperature of 30° or 40° below zero. Solid carbonic acid presents very much the appearance of terrestrial snow, but hitherto it has not been solidified without enormous pressure combined with intense cold. It would doubtless be gratifying to the chemists, and perhaps also to the geologists, to find solid carbonic acid heaped up in piles of snowy whiteness on a neighboring planet; but we shall hesitate to admit such a conclusion, when we find that the force of gravity on Mars is only one half what it is on the earth.

Cyanogen, a compound of two atoms of carbon with one atom of nitrogen, becomes solid at a temperature of thirty degrees below zero of Fahrenheit.

These remarks may suffice to show that the chemists may be called upon to decide respecting questions having important applications to Astronomy.

2. New Tables for determining the Values of those Coefficients in the Perturbative Function of Planetary Motion depending upon the Ratios of the Mean Distances. By J. D. Runkle, Assistant at American Nautical Almanac Office. (By Permission of Superintendent of the Nautical Almanac.)

The first important step in the reduction of the planetary perturbations to numbers, is the determination of those coefficients depending upon the ratios of the mean distances.

This work has been done by different astronomers, but last and most completely by Leverrier, whose results were published in 1841. They do not include Neptune; and besides, Professor Peirce has made changes in some of the mean distances, making a redetermination for the whole system desirable. At Professor Peirce's suggestion, and with the approval of Commander Davis, the Superintendent of the American Ephemeris and Nautical Almanac, with whose sanction these remarks are submitted, I have undertaken this work as part of the systematic labor of a thorough revision of most of the planetary theories, now being carried on in the office of the Nautical Almanac, as fast as can be done consistently with the demands which the regular issues of that work make upon the annual appropriations made by Congress for its support.

If I had merely obtained the desired results, however great the labor, it could hardly have claimed the attention of the Association. But during some preliminary inquiries, I was led to a generalization, which I hope astronomers may receive with indulgence, if not with favor. Each one of these coefficients depends upon a series, arranged according to ascending powers of the ratios of the mean distances, taken less than unity, and usually denoted by a. Leverrier transformed the series given in the theories of Laplace and Legendre into others converging more rapidly, and the coefficients of the different powers of a in these new series have received the name of the *Leverrier coefficients.*

At the request of the Superintendent of the American Nautical Almanac, the values of these coefficients were computed by the late S. C. Walker, assisted by Mr. Pourtalès, and are published in an Appendix to that work for 1857. This carefully prepared paper has been of great aid to me, and especially the manuscript sheets containing the numerical values of the coefficients, which were better adapted to the changes which the form of my tables demanded. If, as usual, we denote the coefficients depending upon a by b_s^i, $D_a b_s^i$, $D_a^2 b_s^i$, &c., we see, since the addition of Neptune and twenty-nine asteroids to the catalogue of known planets, that there are twenty-eight different values of a, for each of which the values of these coefficients must be determined.

The first question which suggested itself was, How do these coefficients vary with a?

Now, it is plain that if this variation is slow, terminating in low orders of differences, we may not only make this circumstance a check upon the accuracy of the work, as far as the different values of any one function is concerned, but we may also tabulate the function with reference to a as an argument, and afterwards enter these tables with the special values of a for the system, and take out the corresponding values of the coefficients.

It was soon found, however, that these variations, instead of being slow, were in most cases so rapid as to make them entirely useless, with any ordinary amount of labor, for the purposes indicated.

If, however, we denote any one of these coefficients by $f(a)$, and write

$$f(a) = \text{a series},$$

may we not find

$$f(a) = f'(a) \text{ (a transformed series)},$$

in which $f'(a)$ is an exact function of a, involving nearly the whole variation of $f(a)$, while the transformed series shall vary so slowly with reference to a, as to be perfectly adapted to the ends already specified? This is the idea which I have found eminently adapted; and, fortunately, only slight and quite obvious changes in Leverrier's series were needed, thus making the valuable labor of Mr. Walker entirely available with corresponding modifications.

With the special value of a, for those planets whose mutual perturbations we wish to estimate, enter the tables and take out the corresponding values of the series, which multiply by $f'(a)$, and we have the value of $f(a)$, the required coefficient.

We conclude, then, that the whole question of computation, transformation of series, rate of approximation, &c., is finally settled. For any subsequent investigations which shall change the values of the mean distances, it will only be necessary to enter the tables with the corrected values of a, and take out anew the corresponding coefficients.

This form of tables is equally adapted to any planets which may hereafter be discovered. Leverrier, with great additional labor, gave these coefficients for three out of four of the old asteroids. My tables give the values for these asteroids with a few hours' labor, and not only for these, but for all which are, or are to be, discovered.

I am authorized by the Superintendent of the Nautical Almanac to say, that these tables will be printed and distributed among astronomers as soon as practicable.

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3. New Tables for converting Longitudes and Latitudes into Right Ascensions and Declinations. By J. D. Runkle, Assistant at American Nautical Almanac Office. (By Permission of Superintendent of the Nautical Almanac.)

A single case of this problem, although not one of the most attractive in Astronomy, presents no great labor; but when it must be repeated hundreds and thousands of times, as is the case especially in the preparation of the Lunar Ephemeris, it becomes excessively tedious, so much so as strongly to suggest the desirableness of finding some means of curtailing the labor of some of its unattractive proportions. For this reason, I have made this problem the subject of more or less study as often as I have been obliged to come in contact with it.

I am not aware of any previous tables for this purpose, except those published by Professor Encke, in the *Jahrbuch*, which, from their form and want of approximation, are entirely unfitted either for rapid work or delicate results.

At last I have hit upon a single table, which, from its form, takes into account all the possible variations of all the elements involved in the problem, with the greatest simplicity and accuracy; and I feel confident that it will reduce its solution to the smallest amount of labor of which it is susceptible. I am authorized by Commander Davis to communicate an explanation of this table to the Association.

If, at equidistant intervals (say 20′) on both sides of the ecliptic, we draw circles of latitude, and compute the right ascensions and declinations of equidistant points on these circles, including the ecliptic, or, in other words, their equatorial co-ordinates, and under the corresponding constant latitudes arrange these co-ordinates to the longitude as an argument, it is evident that we may make the variations of these co-ordinates relatively to the *argument* as small as we please, by taking its intervals sufficiently small. Again, it is equally evident that, with a constant longitude, we may make the variations of these co-ordinates relatively to the latitude as small as we please by taking sufficiently small latitude intervals.

Now, it is found that latitude intervals of 20′ take the differences of the variations of these co-ordinates, or, in other words, their second differences, entirely out of the account; while intervals of 15′ in the longitude argument do the same thing so nearly, that in the same part of the table, where, for extreme accuracy, they may not be neglected, they are so small as to be accounted for with trifling labor.

The following are the order and names of the headings of the different columns of the table for a constant latitude: —

1. Longitude argument.
2. Right ascension.
3. Log. variation of A. R. for 1′ of longitude.
4. Log. variation of A. R. for 1′ of latitude.
5. Log. coefficient of the difference between the obliquity for which the table is constructed, and that of the given date, which we will call $\Delta\omega$.

There are corresponding columns for the declination. All these log. variations are given for the same points as the co-ordinates.

Now, it is evident that, if we enter these tables with a given longitude and latitude, to find the corresponding A. R. and Decl. we must reduce the log. variations for 1′ of longitude to the circle of the given latitude, and the log. variations for 1′ of latitude to the given longitude. But these changes are simple proportional parts of the differences of the log. variations, and only need, on the margin of each page, proportional parts of the differences which it contains.

It is obvious that these proportional parts are equally useful in taking account of second differences whenever it is necessary.

After taking out the log. variations corresponding to the given longitude and latitude, it will only be necessary to multiply them by the excess of these data above their nearest argument values to find the corrections for the tabular A. R. and Decl.

The correction for change in the obliquity needs no further explanation.

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4. Contributions to the Atmospherology of the Sun. By Dr. C. H. F. Peters, of Cambridge.

Our knowledge of the physical constitution of the surface of the sun has made very little or no progress since the time of the elder Herschel. This would prove, either that Herschel's observations are so complete that nothing more remains to be investigated, or that the *central* body of our system has been neglected, while nebulæ and double stars, moon and planets, have been the object of very elaborate researches. Each alternative seems to be the case. By employing similar means, and with the same method, that is to say by simple *contemplation*, it will scarcely be possible to discover anything not already reported by that eminent observer. Only a more favorable sky, as I shall show by example hereafter, may disclose some new and important phenomena. Unfortunately, all the more powerful telescopes are mounted in climates not well adapted for solar observations. But there remains another way of investigating the processes producing those singular changes in the sun's atmosphere, which exhibit themselves as spots, faculæ, and otherwise, a way which has been set aside too much in this subject; I mean by *numerical calculation*. Certainly it is not enough to tell the number of spots for every day, nor to determine occasionally from a larger one the elements of rotation; it seems necessary to follow the spots or faculæ in their relation to the sun's body and to each other, by computing their heliographic positions from exact measurements, made, if possible, from day to day. This way is, indeed, a little troublesome, but nevertheless promises very interesting results.

I have the honor to lay before the Association some conclusions drawn from a series of observations which I began at Naples in the year 1845, especially with the view to ascertain whether on the sun's surface certain fixed localities do exist, where the spots are originating.

I may state beforehand, that all the measurements are taken with the equatorial of the Observatory at Capodimonte, the telescope of which, of four feet focal length and three inches and a half aperture, is a most excellent one, from the hands of Reichenbach. For examining the physical phenomena the ten-foot Fraunhofer was recurred to, provided with a contrivance suggested by Melloni, cutting off the rays of heat, but not disturbing those of light. Of every spot presenting something definite, the difference from the sun's centre was determined in right ascension by repeated transits, and in declination by the three-foot circle, at intervals, when possible, of one or two days. Moreover, the principal systems of spots were sketched, a precaution necessary in order to avoid mistakes in identifying the spots observed on subsequent days. From the so determined differences in A. R. and Decl. the heliocentric situation relative to the sun's equator has been computed, by assuming for the beginning of the year 1846 the longitude of the equator's ascending node upon the ecliptic to be 75° 13′, its inclination 7° 9′, Mr. Laugier's values, which I have found pretty near the truth. Thus are obtained the heliographic latitude and an angle, which I shall call the *argument of latitude;* it is the longitude reckoned from that point of the equator which, at that moment, is in the ecliptic or in the node. But in comparing together different spots observed at different times, it is convenient to establish a first meridian, and (since there is no fixed point always visible upon the sun) I have considered that meridian as the first, which at the beginning of 1846 (Naples mean noon) passed through the ascending node. Then, in order to find the meridian actually passing, a knowledge of the daily motion, or of the time of rotation, is required. The value employed (14° 2′.04 for the daily motion, or $25^{d}.652$ for the time of rotation) has been deduced as a mean from the observations of all the spots. For the following investigation, the first three months have been excluded as being less accurately observed, and moreover disjoined by an interruption of several months. Likewise the observations following October, 1846, were too isolated to be made use of.

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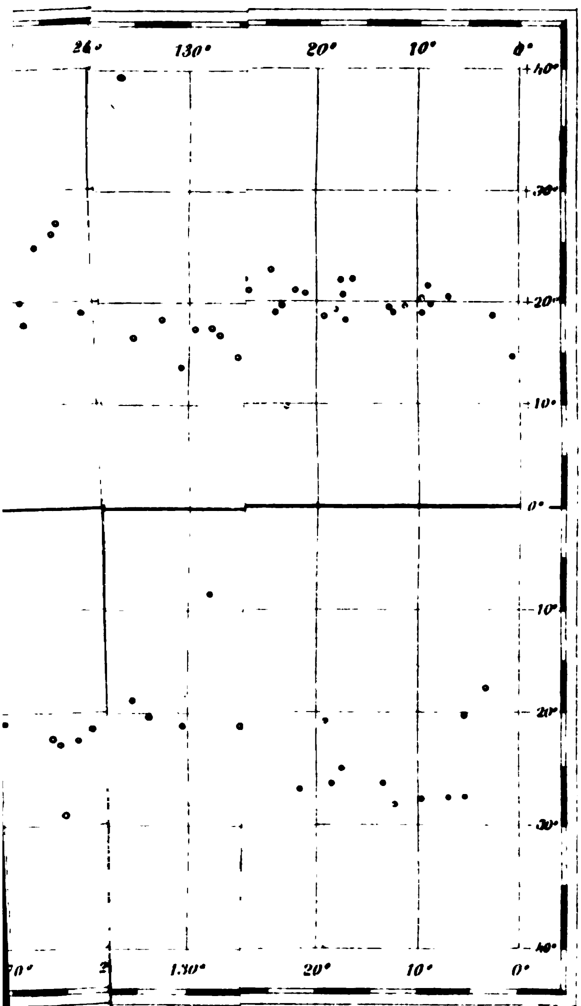


Thus an uninterrupted series of thirteen months remained, from September, 1845, to October, 1846, subministering 813 places of 286 spots. These have been projected in the accompanying map, as far as their mean places.

The first fact, now, which offers itself, in comparing the heliographic places of one and the same spot for different days, is, that the spots are not invariably attached to the sun's surface, but have *proper motions.* Before stating this, due regard should be had to the probable errors that the determination of the compared places may be liable to. This probable error, in my observations, of a difference between a spot and the sun's centre, resulted to be in A. R. 3″ in arc, in Decl. nearly 5″, consequently in all 6″. This would produce in the heliocentric place an error of 215 times 6″ multiplied by the secant of the angle at the sun's centre between earth and spot, 215 being the ratio of the mean distance of the earth to the sun's semidiameter. Near the centre of the sun's disc, then, the probable error would be about 20′; for the angle at the sun's centre equal to 45° it would be 30′, and it would increase rapidly as the spot approaches the limb. I sought to avoid this increase by repeating the observations the more, the nearer the spot was to the limb. And therefore, on the whole, the probable (or more justly speaking the *mean*) error of the heliocentric place as arising from this source may be taken equal to 20′. But a second source we find in the variability of the shape of the spot itself from one day to the other. There is a continual change going on in the spots; less perhaps in the isolated ones, more in those which are in the neighborhood of others, and seem to form part of a system. However, those cases where the change of form was too manifest may be omitted; and we may assume, then, on the average, that two places of a spot as determined on different days ought to agree within half a degree. But now we meet differences of several degrees. Does this not indicate clearly a movement of the spot, especially if, as often, in a *series* of determined places these differences appear to be affected with the same sign?

Wherever in nature a motion is observed, inducement is given to research of laws and of forces causing it. Though, at the first glance, a decrease in latitude seemed predominant, still sometimes this appeared so mixed up with a movement in different sense, that it is only after having carefully examined and combined the circumstances under which every spot was situated, and after having paid special attention to the arising of the individual spots, that I dare to pronounce the two following theses: —

1. *All the spots have a tendency to move towards the equator.*

2. *Whenever a spot is breaking out in the neighborhood of another, the latter is removing itself towards the opposite side.*

The first or general kind of motion, concealed, if the interval of time

TABLE A.

Heliographic Latitudes of Spots which performed an entire Revolution.

(Mean of the determinations in each appearance.)

Spot. No.	No. of Obs.	Mean Epoch of Observation.	Heliographic Latitude.	Spot. No.	No. of Obs.	Mean Epoch of Observation.	Heliographic Latitude.
		d.	° '			d.	° '
6	5	—106.818	—27 21	80	2	+ 93.479	+31 46
286	4	83.568	25 54	81	6	118.718	28 31
				77	4	142.733	24 32
94	6	— 99.442	+19 54				
86	6	76.984	17 12	279	3	+111.394	+18 52
				272	5	136.972	18 37
180	3	— 65.313	—17 30				
177	2	41.566	17 9	244	3	+109.764	—24 6
				232	5	134.401	22 11
151	1	— 34.066	+14 30	210	3	159.661	21 41
147	5	14.780	14 24				
				261	3	+109.764	+21 12
15	7	— 24.777	+18 32	260	2	133.463	20 42
10	7	+ 2.423	18 29				
8	4	24.960	18 4	208	3	+134.649	+17 41
				202	3	159.661	17 42
35	5	+ 7.820	+21 48				
31	2	27.552	19 16	79	1	+176.028	—17 41
				53	5	198.841	16 4
26	3	+ 8.007	+18 0	48	4	224.531	14 27
19	4	27.742	17 38				
				226	5	+244.259	+28 34
229	6	+ 23.018	—18 11	212	1	266.024	26 44
228	2	50.071	17 35				
233	4	78.775	16 50	135	2	+211.034	—21 50
				118	5	233.428	21 7
22	5	+ 29.186	+21 11	120	3	256.356	21 7
18	4	57.725	21 8				
				102	4	+232.256	+36 36
137	3	+ 43.365	+17 39	110	2	255.052	35 25
129	2	67.540	15 56				
119	3	95.634	14 16	273	4	+248.560	+11 32
				264	2	274.921	9 3
249	2	+ 56.579	—15 40				
247	1	74.033	14 40	57	4	+255.288	+13 56
				41	1	278.957	10 27
61	1	+ 91.934	+24 32				
66	4	118.799	23 58	68	3	+254.062	+13 0
				43	1	278.957	10 35

is short, by the disturbed motion, becomes quite obvious in longer intervals, as, for example, in an entire revolution. The annexed Table A contains the latitudes of those spots which reappeared on the eastern limb of the sun's disc after they had disappeared on the western. All the spots, the identity of which could be made out, have been inserted; none excluded. The numbers (which are the means of the single determinations during one appearance, corresponding to the mean epochs) show without exception a decrease in latitude, the unique instance of the increase of one minute being entirely overthrown by the amount of the probable error.

A general proper motion of the spots towards the equator being recognized, the question is raised naturally: Have they any motion also in longitude? and in what sense? to the east or to the west? The solution of this question is intimately connected with the determination of the time of rotation. For it is clear, if all the spots had an *equal* proper motion in longitude, the time of the sun's rotation, since it is deduced from the spots, would be wrong. If the spots move by a certain amount to the west, the sun's rotation will be found too great exactly by that amount; if to the east, it will be too small. In other words, it is the time of rotation of the spots which results, and not that of the sun itself. By employing then the same value in the comparison of observations made at two different epochs, the spot will appear to have been at rest, while it really has changed its place. If the general motion in longitude differs as to its amount for several spots, then a time of rotation will be derived from them, which includes the *average* general motion. Thus the value $25^{d}.652$, which I have given before, and which is made use of for the longitudes in the map, is affected by the average general motion of all the spots observed more than once. By means of this average value of the time of rotation, now, the successive places leave differences so significant that there can be no doubt of a very considerable motion parallel to the equator. The displacements in longitude seem even far more considerable than those in latitude. The annexed Table B gives some examples. Whether there be a common motion, and in what sense, cannot be decided in the present state of our knowledge of the sun's rotation. According to the mode in which this latter is established, the sum of the positive differences always will equal the sum of the negative, whatever be the amount of motion in common, as the

TABLE B.

Table exhibiting the proper Motion of some Spots in Heliographic Longitude.

Epoch.	Longitude.
No. 6 (—27°).	
d.	° ′
—110.042	6 50
109.043	6 12
108.073	5 56
103.987	6 57
102.944	8 8
No. 9 (+21°).	
+55.023	7 58
58.134	8 57
60.018	9 5
62.966	9 28
No. 17 (—26°).	
—110.042	11 9
109.043	12 55
108.073	12 49
103.987	14 29
102.944	15 9
No. 32 (+19°).	
+58.134	25 27
60.018	24 9
62.966	22 45
No. 44 (+24°).	
+167.023	38 2
170.028	39 44
172.019	41 17
174.031	42 52
No. 53 (—16°).	
+194.152	55 5
196.956	52 25
199.028	50 9
201.043	50 42
203.023	49 56
No. 67 (+29°).	
+6.983	58 41
8.015	60 8
9.022	61 13
9.989	62 7
11.013	63 25
No. 72 (+14°).	
d.	° ′
+113.027	68 21
115.033	66 2
118.201	64 48
119.938	64 57
122.025	64 54
No. 81 (+29°).	
+113.027	73 28
115.033	73 27
118.201	75 10
119.938	75 53
122.025	76 27
124.081	77 56
No. 90 (+11°).	
—24.100	91 21
22.044	92 10
20.996	92 47
19.991	89 43
17.990	88 4
No. 105 (+12°).	
—77.037	116 28
74.940	114 48
73.861	113 19
No. 113 (+38°).	
+226.148	115 58
228.036	117 58
231.032	120 46
233.916	122 26
236.041	125 19
238.113	127 22
No. 124 (+17°).	
+91.934	130 39
95.025	129 57
99.942	127 18
No. 130 (—19°).	
+119.938	136 39
122.025	135 45
124.081	135 9
126.087	134 24
No. 144 (+20°).	
d.	° ′
—99.828	163 42
98.963	163 52
97.842	162 11
96.103	160 28
93.169	160 31
91.120	159 32
No. 158 (+28°).	
+258.965	194 27
263.014	195 10
266.024	195 25
270.885	196 13
No. 204 (—28°).	
—38.075	251 44
35.113	251 42
34.066	252 52
32.884	252 53
31.090	253 35
29.086	254 58
No. 219 (+19°).	
+238.113	269 54
241.026	266 14
244.411	264 29
No. 242 (—16°).	
+101.021	292 59
103.026	291 34
106.124	291 10
110.142	289 11
113.027	289 39
No. 275 (+25°).	
+24.960	328 26
27.020	329 9
28.084	329 29
30.902	330 43
No. 286 (—22°).	
—86.029	360 16
84.081	358 30
83.074	357 39
81.089	357 16

greater movements will show an excess, the slower ones a defect, according to phoronomical principles. Nevertheless, there are some reasons which make me incline to the opinion, that the general direction of the movement is towards the *west.* Farther on, I shall return to this question; but in this place, the following observation may be made, namely, that a new spot bursts out almost always to the *east* of an older one, very seldom to the west. Since it is of some interest, the linear velocity has been computed for some of the spots which seemed imbued with a somewhat considerable motion (as on the annexed Table C). Here are velocities up to between 300 and 400 miles the hour, which is not so extraordinary, considering that the sun's diameter is 112 times that of the earth, and that the strongest hurricanes on the earth are supposed to possess a velocity of 120 miles an hour. For the sake of comparison, it may be mentioned that the velocity of rotation of a point at the sun's equator is between four and five times as great as of a point at the earth's equator; or as the latter is nearly 900 miles, the former is about 4,000 miles, the hour.

TABLE C.

Some of the greater Absolute Velocities per hour of Solar Spots observed in Geographical Miles.

Spot.	Velocity.	Spot.	Velocity.
No. 12	20	No. 273	109
232	29	144	129
215	43	230	129
202	57	17	132
22	62	82	153
118	63	286	168
132	73	95	169
53	75	113	207
242	84	35	222
19	92	88	238
204	98	90	261
81	102	67	277
66	106	219	366

The second fact which I wish to draw attention to, results by combining together the heliographic positions of different spots, and may be pronounced, for the present, in the following thesis: —

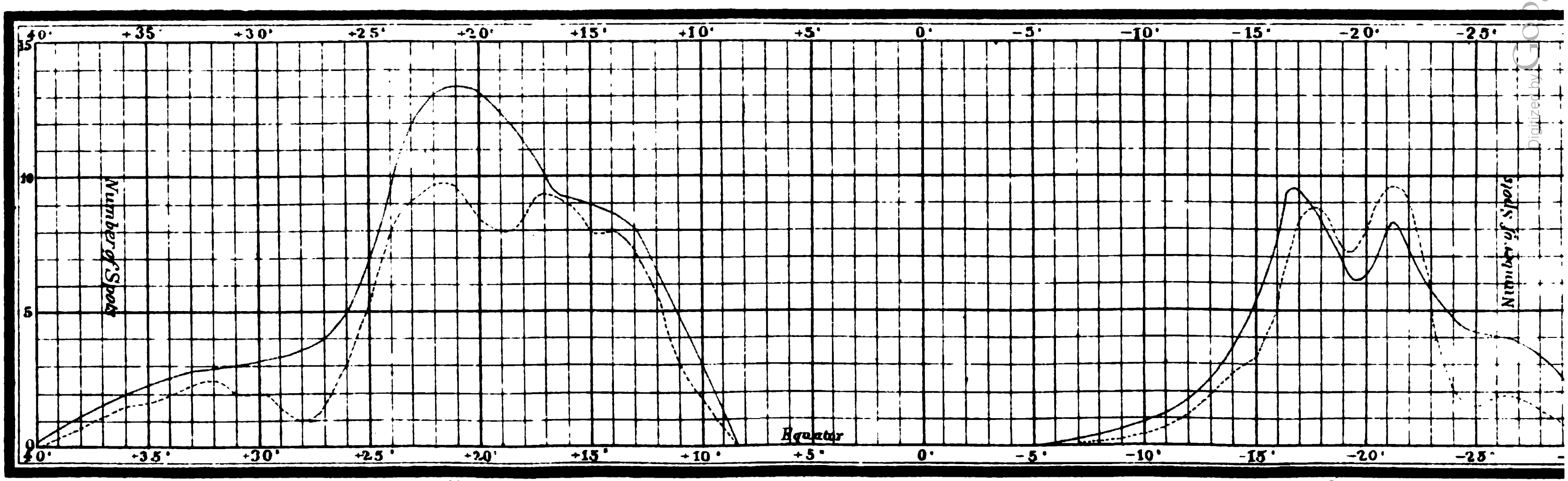
In certain localities of the solar surface, the spots arise more frequently than in others.

A glance upon the map shows immediately that the spots are arranged in two girdles or belts, one to each side of the equator.

This observation is not entirely new. Already Galileo had stated, that all the spots are found within an equatorial zone, the limits of which he fixed at 29°. The Jesuit Scheiner extended these limits to 30°; Messier, to 31°; Lalande, Delambre, and Méchain, to 40°; Laugier, to 41°, north and south. The remark, that the neighborhood of the equator itself is barren of spots, is of a more recent date. I see that Sir John Herschel, in his "Cape Observations," has come to a similar conclusion. Still, this region, too, is not entirely destitute of the capability of producing spots. In March, 1845, I saw some so near to the equator as 3°. But these are rather rare. In regard to the outer limits of the zone, perhaps the same may be said. By Lahire, a spot is related in 70° latitude. I observed one during several days in 50° 24′, and a remarkable facula as high up as 67°. Of the northern spots, inserted in the map, the greatest number is near to the parallel of 21°; of the southern, the greatest number is near to 17°. Here it is proper to make a distinction between *individual* spots, and *systems* or groups of spots. The spots forming a system show clearly an identical perturbation generating them in the same moment. The relative frequency in different parallels of latitude of the individual spots, as well as of the systems of spots, is represented by the curves in the annexed drawing. (Plate I.)

The establishing of *both* the co-ordinates of some fixed points as particularly productive of spots, is certainly of the greatest importance. The observations of thirteen months have shown themselves insufficient to be decisive as to the positions in longitude, though they give some intimations. The great obstacle is the uncertainty about the time of rotation, as including the proper motion in longitude. Observations continued during four or five years, and duly computed, probably would carry us a considerable step farther in this research, and consequently nearer to the true philosophy of the sun's nature. At all events, it is reasonable to suppose that the proper motion is only small in proportion to the rotary motion, so that the latter may be assumed to be right at least *approximately*. With this assumption, my computations give several instances of spots breaking out at *nearly* the same place after intervals of two and three hundred days, whilst in the intermediate time that place was plain and smooth like the rest of the sun's surface, not indicating any sign of disturbance. Avoiding, for obvious reasons, those parallels where the appearances are more condensed, I wish to point out as examples:

Curves showing the relative frequency of Spots in different heliographical Latitudes as observed in the years 1845-46.



Note. *The dotted Curve shows the* double *number of* Systems *of Spots.*

280°	long.	and	+30°	lat.
200°	"	"	+12°	"
115°	"	"	+35°	"
15°	"	"	—28°	"

In order to justify the assumption, that our knowledge of the time of rotation cannot be very much out of the way, and that, of course, the proper motion of the spots can be but a small fraction of the rotary motion, I may mention the results obtained by two modern philosophers, who attempted to derive the time of the sun's rotation from observations of terrestrial temperatures. The value deduced from the long series of Innsbruck and Paris temperatures by Professor Nervander, of Helsingfors, is 27.26 days for the synodical, or 25.367 for the tropical revolution. Mr. Buys-Ballot, in Utrecht, in the same manner making use of the Belgian and other thermometrical observations, gives 27.692 days for the synodical, which would be 25.742 for the sidereal revolution.* I made some trials, to arrive at a direct conclusion of the rotation by other ways, which, for a longer series of observations, will be found effective. One is based upon the similitude of certain figures of the spots when they are first outbursting. Among these figures is an elliptic arrangement not uncommon, and two pair of those observed by me may well be considered as arisen in the same locality. A second way would be precisely by the assumption of the identity of those places, which I indicated before. But their number is still too small, and the uncertainty deriving from the extent of those localities too great, for deriving from them any exact result.

After having exposed several facts, which result from the numerical investigation of measurements, I may now relate briefly some telescopic observations, one of which, favored by the beautiful sky of Naples, is worthy the particular attention of other observers. In order to make better understood this phenomenon, I ought to trace shortly the biography of spots, such as on the whole I have noted it in the suc-

* These two results differ to about the same amount as the determinations by means of spots. The mean value of Delambre's and Lalande's researches is 25.021 days; Laugier found from 29 spots 25.34 days, the particular spots varying from 24.28 to 26.23 days; Dr. Böhm formed equations of condition for 13 spots, which gave 25.821 with a probable error of only 0.024 days. The value which I have adopted as a preliminary one, from 286 spots, 25.652 days, is intermediate between all these different determinations.

cessive stages of their appearance. The spots arise from insensible points, so that the exact moment of their origin cannot be stated; but very rapidly they grow in the beginning, and almost always in less than a day they arrive at their maximum of size. Then they are stationary, I would say in the vigorous epoch of their life, with a well defined penumbra, of regular and rather simple shape. So they sustain themselves for ten, twenty, and some even for fifty days. Then the notches of the margin, which, seen with a high magnifying power, always appears somewhat serrate, grow deeper, to such a degree that the penumbra in some parts becomes interrupted by straight and narrow luminous tracts; — already the period of decadence is approaching. This begins with the following highly interesting phenomenon. Two of the notches from opposite sides step forward into the area, overroofing even a part of the nucleus; and suddenly from their prominent points flashes go out, meeting each other on their way, hanging together for a moment, then breaking off and receding to their points of starting. Soon this electric play begins anew, and continues for a few minutes, ending finally with the connection of the two notches, thus establishing a bridge, and dividing the spot in two parts. Only once I had the fortune to witness the occurrence between *three* advanced points. Here, from the point *A* a flash proceeded towards *B*, which sent forth a ray to meet the former, when this had arrived very near. Soon this seemed saturated, and was suddenly repelled; however, it did not retire, but bent with a rapid swing towards *C*; then again, in the same manner, as by repulsion and attraction, it returned to *B*, and after having oscillated thus for several times, *A* adhered at last permanently to *B*. The flashes proceeded with great speed, but not so that the eye might not follow them distinctly. By an estimation of time and the known dimension of space traversed, at least an *under* limit of the velocity may be found; thus I compute this velocity to be not less than two hundred millions of metres (or about one hundred thousand miles) in a second. The process described is accomplished in the higher photosphere, and seems not at all to affect the lower or dark atmosphere. With it a second, or rather third, period in the spot's life has begun, that of dissolution, which lasts sometimes for ten or twenty days; during which time the components are again subdivided, whilst the other parts of the luminous margin too are pressing, diminishing, and finally over-casting the whole, — thus ending the ephemeral existence of the spot. Rather a good chance is required for observing the remarkable phenomenon which introduces the covering process, since it is achieved in a few minutes, and it demands, moreover, a perfectly calm atmosphere, in order not to be confounded with a kind of scintillation, which is perceived very often in the spots, especially with fatigued eyes. The observer ought to watch for it, under otherwise favorable atmospherical circumstances, when a large and ten or twenty days' old spot begins to show strong indentations on its margin.

In the foregoing exposition I have avoided carefully all theoretical speculations and hypotheses. I have brought before you the facts simply as they resulted from observations combined by vigorous numerical computations. However insignificant these single results may appear, still they may lead to some important consequences relative to our knowledge of the nature of the sun. And as the method generally employed in natural philosophy consists in building upon a few facts a hypothesis as to the cause, which hypothesis then, by other facts, ought to be tried, and either rejected or confirmed, thus also to the astronomer it may be permitted, in an object which, until now, was rather more physical, than within the reach of quantitative determination.

At present there is no more any doubt about the view of Herschel (or rather Wilson) with regard to the constitution of the sun in general. Above the solid and opaque nucleus rises the atmosphere, of a nature perhaps similar to ours; this again is surrounded by a second atmosphere, luminous, and very thin. The spots are openings in both the atmospheres, leaving visible the dark solar body; the penumbra is the limb of the lower atmosphere. But now, with regard to the origin of the spots, Herschel seems to have inclined to an idea which is neither natural nor corresponding to the phenomena. According to him, the atmosphere is set in strong motion, by a cause, which he does not define nearer, is then thrown against high mountains, and thus the openings in the luminous matter are produced. This idea is indeed a little strange; and it seems to have been abandoned by modern philosophers, though without advancing any other theory in its place. All the facts of observation have impressed me strongly with the supposition, that on the surface of the solar body something exists similar to *volcanoes*, breaking out and emitting gaseous matter, in a manner similar to the terrestrial volcanoes. These gases pass easily and speedily through the lower or dark atmosphere, until they arrive at the photosphere. Of this we have nothing in the least degree analogous upon our planet; but it appeared to me, under all circumstances, to possess in a high degree the propriety of viscosity. This viscosity at first opposes the gases, and faculæ are formed. But the gases succeed in overcoming the coherency, breaking the photosphere from beneath, usually first in numerous small points, which very soon become larger, as in the mean time the gaseous matter had collected, now bursting out with greater elasticity. These gases really have been seen; they have been observed outside of the photosphere; they are identical with the *rosy light* in the total eclipses of the sun. Especially the eclipse of July, 1851, has left no doubt, as well about the cloudy nature of this light as about its connection with faculæ and spots. Are the purple-colored ridges, which sometimes have been seen on the dark ground of the spots, perhaps also nothing else than the gases elevated above the photosphere, illuminated, and seen by projection upon the dark nucleus? With a very clear sky, the bottom of the larger spots appears never black, but always brownish or purplish, the outlines of the ridges widened, less distinct, outwashed, and the ridges themselves resemble then stronger colored plots. The gaseous exhalations explain completely the appearances of the faculæ, inflated bubbles covered by the viscous luminous matter, always preceding the outbreak of a spot, surrounding it in its first stage, but then disappearing. When the gases are effecting their escape, the luminous matter is pressed aside; hence the removal of a neighboring spot, agreeably to observation. The very sudden increase after the first opening is made, the then following stationary size, the reopening of spots nearly closed by the covering process, are necessary consequences of the emanation of elastic matters. The gases start from the surface of the nucleus impressed with the velocity of rotation corresponding to that point; in ascending to higher regions, where the velocity of rotation is greater, they remain behind to the west; on reaching the photosphere, they are opposed by the viscous tenacity of the latter, by overpowering which they lose a part of their vertical velocity, and press towards the west, communicating this direction of motion to the luminous matter surrounding the spot, and of course to the spot itself. The observation I have before made, namely, that a new spot breaks forth in most cases on the *east* side of an older one, is now accounted for by a repeated eruption of the volcano, after it had been reposing for a short lapse of time, during which the older opening has advanced towards the west. The volcanoes are situated principally in two zones or belts, on both sides of the equator, to which phenomenon geologists will find an analogy in the greater mountain chains upon our globe; hence the frequency of spots on certain parallels. If this theory of volcanic eruptions which I have dared to advance shall be confirmed, especially by the establishment of certain fixed eruptive points in longitude, then, with some confidence, we may compare the moon to a country of extinguished volcanoes, our planet to a volcano languishing in eruptions, and the sun to the seat of volcanoes in a prodigious state of activity.

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5. Method of determining Longitudes by Occultations of the Pleiades. By Professor Benjamin Peirce, of Cambridge. (Communicated by Permission of Professor A. D. Bache, Superintendent of the Coast Survey.)

1. The determination of longitudes by occultations of the stars appears to be the most exact of all astronomical methods for such determinations, and deserves therefore a most careful examination, in order to ascertain the greatest degree of accuracy of which it is susceptible, and the surest method of securing such accuracy. The sources of error are partly those of observation, and partly those of theory. The errors arising from observation are of two classes; first, there are those which are special to the observations of the occultations; and, secondly, there are those which are general and inseparable from the theoretical defects.

2. The probable error of the direct observation of an occultation has been investigated by Commander C. H. Davis, from simultaneous observations made by different observers at the same place. From his researches, which were communicated to the Association at Washington, it appears that this probable error is about a fifth of a second of time, so that the ultimate probable error of the mean of this class

of observations cannot exceed a twentieth of a second of time. If, therefore, the theoretical defects can be eliminated by proper precautions and a sufficient accumulation of observations, longitudes may be obtained by this method, of which the probable error shall be decidedly inferior to a tenth of a second of time.

3. It is obvious that, with the present uncertainty of the lunar theory, isolated occultations cannot approach this degree of accuracy in the determination of longitudes. But well-determined groups of stars are essential to correct the lunar elements and rectify the places of the stars themselves. The present plan is to carry out the suggestions of Walker, in his report to the Superintendent published in 1851, by combining all the known observations of occultations of the Pleiades, and using them to correct the lunar semidiameter, the mutual positions and changes of position of the stars of this group, to test and correct the formulæ for lunar parallax, to determine the irregularities of the moon's limb, and, finally, to correct the longitudes of the places of observation.

4. Of the various forms of computation which might be adopted, I have selected that which is derived from the stereographic projection of the sphere, in which the star Alcyone *is the pole of projection.* The advantages of the stereographic projection consist in the circularity of the projections of all the spherical circles, so that the moon is represented by a circle on the plane of projection. The advantage of placing the pole of projection at the star Alcyone is, that the distances and relative positions of the projected places of the stars are only affected by the differences of their proper motions, and the small differential effects of aberration. There may be a doubt whether the somewhat greater simplicity of the formulæ, in the case in which the pole of projection coincides with that of the celestial equator, should not cause this form of projection to be preferred; and it may be advisable, in order to insure accuracy, to conduct the computations, independently, by each method.

5. As the basis of computation, the places of the stars have been taken from Bessel's investigations, which are contained in the first volume of his *Astronomische Untersuchungen*, in the article entitled *Beobachtungen Verschiedener Sterne der Plejaden.* The places of the moon are taken from the Nautical Almanac, or from the "Tables of the Moon," constructed for the American Ephemeris, and the

moon's parallax and semidiameter from the "Tables of the Moon's Parallax constructed from Walker's and Adams's Formulæ for the Use of the American Ephemeris and Nautical Almanac."

6. The following are the formulæ for the computation of the stereographic projections: —

Let α = the right ascension of Alcyone;
β = the declination of Alcyone;
α' = the right ascension of another star or of the moon's centre;
β' = the declination of the second star or of the moon's centre;
$\Delta\alpha = \alpha' - \alpha$;
$\Delta\beta = \beta' - \beta$.

The axes of x and y have their origin at Alcyone, the axis of y is directed to the north, and that of x to the east. The co-ordinations of the star are given by the formulæ,

$$A = 1 - \sin^2 \tfrac{1}{2} \Delta\beta + \sin^2 \tfrac{1}{2} \Delta\alpha \cos\beta \cos\beta'$$
$$B \sin 1'' = \sin \Delta\alpha \cos\beta';$$
$$C \sin 1'' = \Delta\beta + 2 \sin^2 \tfrac{1}{2} \Delta\alpha \sin\beta \cos\beta';$$
$$x = \frac{B}{A};$$
$$y = \frac{C}{A}.$$

The radius of the circle, which represents the moon, is given by the formula,

$$\Sigma_2 = [1 + \tfrac{1}{4} (x^2 + y^2) \sin^2 1''] \, \Sigma_1,$$

in which Σ_1 is the augmented semidiameter of the moon.

The computation of A, C, and Σ_2 should be performed with the aid of the Gaussian logarithms.

7. The formulæ for the corrections of latitude for the earth's eccentricity are,

ϕ = the geographical latitude of the place;
e = the earth's eccentricity;
$\sin\psi = e \sin\phi$;
$h = \sec\psi \cos\phi$;
$k = (1 - e^2) \sec\psi \sin\phi$.

8. The parallax of the moon in right ascension and declination, and

its augmented semidiameter, are obtained from the formulæ of Olbers, which are,

π = the moon's equatorial horizontal parallax;
s = the sidereal time at the place of observation;
a_0 = the moon's tabular right ascension;
β_0 = the moon's tabular declination;
$\Delta_\pi a = a' - a_0$ = the parallax in right ascension;
$\Delta_\pi \beta = \beta' - \beta_0$ = the parallax in declination;

$$P \sin 1'' = h \sin \pi \sec \beta_0;$$

$$\text{tang}\, \Delta_\pi a = \frac{P \sin 1'' \sin (s - a_0)}{1 - P \sin 1'' \cos (s - a_0)};$$

$$\text{tang}\, \eta = \frac{k \cos \frac{1}{2} \Delta_\pi a}{h \cos (s - a_0 - \frac{1}{2} \Delta_\pi a)};$$

$$Q \sin 1'' = \frac{k \sin \pi}{\sin \eta};$$

$$\text{tang}\, \Delta_\pi \beta = \frac{Q \sin 1'' \sin (\beta_0 - \eta)}{1 - Q \sin 1'' \cos (\beta_0 - \eta)};$$

$$\log a = 9.435000;$$

$$\Sigma_1 = a \pi \frac{\sin (\beta' - \eta)}{\sin (\beta_0 - \eta)}.$$

9. In order to determine the equations of condition for the correcting of the lunar elements, of the places of the stars, and of the longitude of the place, let

x_m, y_m denote the co-ordinates of the moon's place affected with parallax;
x_s, y_s, those of the star's place;
p, the distance of the star from the centre of the moon for the recorded instant of the observed immersion or emersion;
θ, the angle which p makes with the axis of x;
θ', the angle which the moon's apparent path, affected with parallax, makes with the axis of x;
v, the velocity of the moon for a second of time estimated in seconds of space;
x'_m, y'_m, the changes in the values of x_m and y_m for a second of time;
δx_m, the correction of x_m for the instant denoted by τ;
$\delta \beta_m$, the correction of the moon's declination for the instant τ;
δx_s, the correction of x_s for the year 1840;

$\delta \beta_s$, the correction of the star's declination for the year 1840;
$\delta x'_m$, the correction of the hourly change of x_m;
$\delta \beta'_m$, the correction of the moon's hourly motion in declination;
$\delta x'_s$, the correction of the star's annual change of x_s;
$\delta \beta'_s$, the correction of the star's annual proper motion in declination;
$\delta \pi$, the correction of the moon's horizontal parallax;
δa, the correction of a;
δb, the correction of the moon's semidiameter for irregularity of outline;
$\delta \lambda$, the correction of the western longitude of the place in seconds of time;
δt, the correction in seconds of the local time of observation for the night's work;
t, the time expressed in hours and decimals of an hour;
t_y, the time in years from 1840.

The subsidiary formulæ for the determination of p, v, θ, and θ' are,

$$p \cos \theta = x_s - x_m;$$
$$p \sin \theta = y_s - y_m;$$
$$v \cos \theta' = x'_m;$$
$$v \sin \theta' = y'_m.$$

And the equation of condition is

$$\cos \theta \left[\delta x_s - \delta x_m + t_y \delta x'_s - (t - \tau) \delta x'_m\right] + \sin \theta \left[\delta \beta_s - \delta \beta_m + t_y \delta \beta'_s - (t - \tau) \delta \beta'_m\right] - \left[\frac{\Delta_\pi a \cos \beta_0}{\pi} \cos \theta + \frac{\Delta_\pi \beta}{\pi} \sin \theta + a\right] \delta \pi - \pi \delta a - \delta p - v \cos (\theta' - \theta) \left[\delta \lambda + \delta t\right] = \Sigma_2 - p.$$

10. In computing x_m and y_m by the formulæ of § 6, the apparent right ascension and declination of Alcyone must be taken for the time from the Nautical Almanac or Bessel's *Tabulæ Regiomontanæ.* The values of x_s and y_s must be corrected for proper motion, and also for the change in the co-ordinates arising from precession and aberration. The formulæ for the computation of these changes have been recently investigated by Dr. Peters, who has constructed tables which greatly facilitate their use.

(Dr. Peters's investigation was here presented to the Association, and also the co-ordinates of the Pleiades, computed by Mr. Webber and myself, as well as those of the values of h and k, for all the principal observatories.)

11. The great number of corrections to be determined will prevent the success of this method, unless they are divided, with just discretion, into classes arranged for separate discussion. Thus the determination of the irregularities of the moon must be preceded by that of its mean diameter, and the proper motions of the stars cannot be corrected until the final combination of all the observations. For each night of observation, the elements of the moon's place, its parallax, and the local time, must be subjected to separate discussion, while for each period of occultation the corrections of the star's places, of the longitudes, and of the constant *a*, may be determined.

This labor will be greatly relieved by a new determination of the places of the Pleiades, and there seems to be no instrument capable of such delicate work but the heliometer. It is much to be desired, therefore, that this important instrument may be obtained for one of our observatories.

NOTE. — Dr. Armsby of Albany, who was present at the meeting, was incited by this announcement to write to Mr. Olcutt, who immediately undertook to obtain the aid of the gentlemen of Albany in providing this instrument for the Dudley Observatory of Albany. But upon submitting the matter to Mrs. Dudley, this generous lady offered to take the whole burden upon herself. By the ardent zeal of other gentlemen, aided by the generosity of another lady, Mrs. Corning, Dr. Gould was, in a few weeks, induced to go to Europe for the purpose of purchasing, for the Dudley Observatory, the Dudley Heliometer, the Corning Astronomical Clock, a meridian circle, and a prime-vertical transit instrument. The magnitude and promptness of this liberality are equally remarkable, and cannot fail to stimulate the Association to perseverance in its efforts for the advancement of science.

III. PHYSICS.

1. On the Mode of testing Building Materials, and an Account of the Marble used in the Extension of the United States Capitol. By Professor Joseph Henry, Secretary of the Smithsonian Institution.

A commission was appointed by the President of the United States in November, 1851, to examine the marbles which were offered for the extension of the United States Capitol, which consisted of General Totten, A. J. Downing, the Commissioner of Patents, the architect, and myself. Another commission was subsequently appointed, in the early part of the year 1854, to repeat and extend some of the experiments, — the members of which were General Totten, Professor Bache, and myself.

A part of the results of the first commission were given in a report to the Secretary of the Interior, and a detailed account of the whole of the investigations of these committees will ultimately be given in full in a report to Congress, and I propose here merely to present some of the facts of general interest, or which may be of importance to those engaged in similar researches.

Though the art of building has been practised from the earliest times, and constant demands have been made, in every age, for the means of determining the best materials, yet the process of ascertaining the strength and durability of stone appears to have received but little definite scientific attention, and the commission, who have never before made this subject a special object of study, have been surprised with unforeseen difficulties at every step of their progress, and have come to the conclusion that the processes usually employed for solving these questions are still in a very unsatisfactory state.

It should be recollected, that the stone in the building is to be exposed for centuries, and that the conclusions desired are to be drawn from results produced in the course of a few weeks. Besides this, in the present state of science, we do not know all the actions to which the materials are subjected in nature, nor can we fully estimate the amount of those which are known.

The solvent power of water, which even attacks glass, must in time produce an appreciable effect on the most solid material, particularly where it contains, as the water of the atmosphere always does, carbonic acid in solution. The attrition of siliceous dusts, when blown against a building, or washed down its sides by rain, is evidently operative in wearing away the surface, though the evanescent portion removed at each time may not be indicated by the nicest balance. An examination of the basin which formerly received the water from the fountain at the western entrance of the Capitol, now deposited in the Patent Office, will convince any one of the great amount of action produced principally by water charged with carbonic acid. Again, every flash of lightning not only generates nitric acid,—which, in solution in the rain, acts on the marble,—but also by its inductive effects at a distance produces chemical changes along the moist wall, which are at the present time beyond our means of estimating. Also the constant variations of temperature from day to day, and even from hour to hour, give rise to molecular motions which must affect the durability of the material of a building. Recent observations on the pendulum have shown that the Bunker Hill Monument is scarcely for a moment in a state of rest, but is constantly warping and bending under the influence of the varying temperature of its different sides.

Moreover, as soon as the polished surface of a building is made rough from any of the causes aforementioned, the seeds of minute lichens and mosses, which are constantly floating in the atmosphere, make it a place of repose, and by the growth and decay of the microscopic plants which spring from these, discoloration is produced, and disintegration assisted.

But perhaps the greatest source of the wearing away in a climate like ours, is that of the alternations of freezing and thawing which take place during the winter season; and though this effect must be comparatively powerful, yet, in good marble, it requires the accumulated effect of a number of years in order definitely to estimate its amount. From all these causes, the commission are convinced that the only entirely reliable means of ascertaining the comparative capability of marble to resist the weather, is to study the actual effects of the atmosphere upon it, as exhibited in buildings which for years have been exposed to these influences. Unfortunately, however, in this country, but few opportunities for applying this test are to be found. It is true some analogous information may be derived from the examination of the exposed surfaces of marble in their out-crops at the quarry; but in this case the length of time they have been exposed, and the changes of actions to which they may have been subjected during, perhaps, long geological periods, are unknown; and since different quarries may not have been exposed to the same action, they do not always afford definite data for reliable comparative estimates of durability, except where different specimens occur in the same quarry.

As we have said before, the art of testing the quality of stone for building purposes is at present in a very imperfect state; the object is to imitate the operations of nature, and at the same time to hasten the effect by increasing the energy of the action, and, after all, the result may be deemed but as approximative, or, to a considerable degree, merely probable.

About twenty years ago an ingenious process was devised by M. Brard, which consists in saturating the stone to be tested with a solution of the sulphate of soda. In drying, this salt crystallizes and expands, thus producing an exfoliation of surface which is supposed to imitate the effect of frost. Though this process has been much relied on, and generally employed, recent investigations made by Dr. Owen lead us to doubt its perfect analogy with that of the operations of nature. He found that the results produced by the actual exposure to freezing and thawing in the air, during a portion of winter, in the case of the more porous stones, produced very different results from those obtained by the drying of the salt. It appears from his experiments, that the action of the latter is chemical as well as mechanical.

The commission, in consideration of this, have attempted to produce results on the stone by freezing and thawing by means of artificial cold and heat. This process is, however, laborious; each specimen must be inclosed in a separate box fitted with a cover, and the amount of exfoliation produced is so slight, that in good marble the operation requires to be repeated many times before reliable comparative results can be obtained. In prosecuting this part of the inquiries, unforeseen difficulties have occurred in ascertaining precisely the amount of the disintegration, and it has been found that the results are liable to be vitiated by circumstances which were not foreseen at the commencement of the inquiries.

It would seem at first sight, and the commission when they undertook the investigation were of the same opinion, that but little difficulty would be found in ascertaining the strength of the various specimens of marbles. In this, however, they were in error. The first difficulty which occurred was to procure the proper instrument for the purpose. On examining the account of that used by Rennie, and described in the Transactions of the Royal Society of London, the commission found that its construction involved too much friction to allow of definite comparative results. Friction itself has to be overcome, as well as the resistance to compression, and since it increases in proportion to the pressure, the stronger stones would appear relatively to withstand too great a compressing force.

The commission first examined an instrument — a hydraulic press — which had previously been used for experiments of this kind, but found that it was liable to the same objection as that of the machine of Rennie. They were, however, extremely fortunate subsequently in obtaining, through the politeness of Commodore Ballard, commandant of the Navy Yard, the use of an admirable instrument devised by Major Wade, late of the United States Army, and constructed under his direction, for the purpose of testing the strength of gun metals. This instrument consists of a compound lever, the several fulcra of which are knife-edges, opposed to hardened steel surfaces. The commission verified the delicacy and accuracy of the indications of this instrument by actual weighing, and found, in accordance with the description of Major Wade, the equilibrium was produced by *one* pound in opposition to *two hundred.* In the use of this instrument the commission were much indebted to the experience and scientific knowledge of Lieutenant Dahlgreen, of the Navy Yard, and to the liberality with which all the appliances of that important public establishment were put at their disposal.

Specimens of the different samples of marble were prepared in the form of cubes of one inch and a half in dimension, and consequently exhibiting a base of two and a quarter square inches. These were dressed by ordinary workmen with the use of a square, and the opposite sides made as nearly parallel as possible by grinding by hand on a flat surface. They were then placed between two thick steel plates, and, in order to insure an equality of pressure, independent of any want of perfect parallelism and flatness on the two opposite surfaces, a thin plate of lead was interposed above and below between the stone and the plates of steel. This was in accordance with a plan adopted by Rennie, and that which appears to have been used by most, if not all, of the subsequent experimenters in researches of this kind. Some doubt, however, was expressed as to the action of interposed lead, which induced a series of experiments to settle this question, when the remarkable fact was discovered, that the yielding and approximately equable pressure of the lead caused the stone to give way at about half the pressure it would sustain without such an interposition. For example, one of the cubes, precisely similar to another which withstood a pressure of upwards of 60,000 pounds when placed in immediate contact with the steel plates, gave way at about 30,000 with lead interposed. This remarkable fact was verified in a series of experiments, embracing samples of nearly all the marbles under trial, and in no case did a single exception occur to vary the result.

The explanation of this remarkable phenomenon, now that it is known, is not difficult. The stone tends to give way by bulging out in the centre of each of its four perpendicular faces, and to form two pyramidal figures, with their apices opposed to each other at the centre of the cube, and their bases against the steel plates. In the case where rigid equable pressure is employed, as in that of the thick steel plate, all parts must give way together. But in that of a *yielding* equable pressure, as in the case of interposed lead, the stone first gives way along the lines of least resistance, and the remaining pressure must be sustained by the central portions around the vertical axis of the cube.

After this important fact was clearly determined, lead and all other interposed substances were discarded, and a method devised by which the upper and lower surfaces of the cube could be ground into perfect parellelism. This consists in the use of a rectangular iron frame, into which a row of six of the specimens could be fastened by a screw at the end. The upper and lower surfaces of this iron frame were wrought into perfect parallelism by the operation of a planing machine. The stones being fastened into this, with a small portion of the upper and lower parts projecting, the whole were ground down to a flat surface, until the iron and the face of the cubes were thus brought into a continuous plane. The frame was then turned over, and the opposite surfaces ground in like manner. Care was of course taken that the surfaces thus reduced to perfect parallelism, in order to receive the action of the machine, were parallel to the natural beds of the stone.

All the specimens tested were subjected to this process, and in their exposure to pressure were found to give concordant results. The crushing force exhibited in the subjoined table is much greater than that heretofore given for the same material.

The commission have also determined the specific gravities of the different samples submitted to their examination, and also the quantity of water which each absorbs.

They consider these determinations, and particularly that of the resistance to crushing, tests of much importance, as indicating the cohesive force of the particles of the stone, and its capacity to resist most of the influences before mentioned.

The amount of water absorbed may be regarded as a measure of the antagonistic force to cohesion, which tends, in the expansion of freezing, to disintegrate the surface. In considering, however, the indication of this test, care must be taken to make the comparison between marbles of nearly the same texture, because a coarsely crystallized stone may apparently absorb a small quantity of water, while in reality the cement which unites the crystals of the same stone may absorb a much larger quantity. That this may be so was clearly established in the experiments with the coarsely crystallized marbles examined by the commission. When these were submitted to a liquid which slightly tinged the stone, the coloration was more intense around the margin of each crystal, indicating a greater amount of absorption in these portions of the surface.

The marble which was chosen for the Capitol is a dolomite, or is composed of carbonate of lime and magnesia in nearly atomic proportions. It was analyzed by Dr. Torrey of New York, and Dr. Genth of Philadelphia. According to the analysis of the former, it consists, in hundredth parts, of

Carbonate of lime,	54.621
Carbonate of magnesia,	43.932
Carbonate of protoxide of iron,	.365
Carbonate of protoxide of manganese,	(a trace.)
Mica,	.472
Water and loss,	.610

The marble is obtained from a quarry in the southeasterly part of the town of Lee, in the State of Massachusetts, and belongs to the great deposit of primitive limestone which abounds in that part of the district. It is generally white, with occasional blue veins. The structure is fine-grained. Under the microscope it exhibits fine crystals of colorless mica, and occasionally also small particles of bisulphuret of iron. Its specific gravity is 2.8620; its weight 178.87 lbs. per cubic foot; it absorbs .103 parts of an ounce per cubic inch, and its porosity is great in proportion to its power of resistance to pressure. It sustains 23,917 lbs. to the square inch. It not only absorbs water by capillary attraction, but, in common with other marbles, suffers the diffusion of gases to take place through its substance. Dr. Torrey found that hydrogen and other gases, separated from each other by slices of the mineral, diffuse themselves with considerable rapidity through the partition.

This marble, soon after the workmen commenced placing it in the walls, exhibited a discoloration of a brownish hue, no trace of which appeared so long as the blocks remained exposed to the air in the stonecutter's yard. A variety of suggestions and experiments were made in regard to the cause of this remarkable phenomenon, and it was finally concluded that it was due to the previous absorption by the marble of water holding in solution a small portion of organic matter, together with the absorption of another portion of water from the mortar.

To illustrate the process, let us suppose a fine capillary tube, the lower end of it immersed in water, and of which the internal diameter is sufficiently small to allow the liquid to rise to the top, and be exposed to the atmosphere; evaporation will take place at the upper surface of the column, a new portion of water will be drawn in to supply the loss; and if this process be continued, any material which may be dissolved in the water, or mechanically mixed with it, will be found deposited at the upper orifice of the tube, or at the point of evaporation.

If, however, the lower portion of the tube be not furnished with a supply of water, the evaporation at the top will not take place, and the deposition of foreign matter will not be exhibited, even though the tube itself may be filled with water impregnated with impurities. The pores of the stones so long as the blocks remain in the yard are in the condition of the tube not supplied at its lower end with water, and consequently no current takes place through them, and the amount of evaporation is comparatively small; but when the same blocks are placed in the wall of the building, the absorbed water from the mortar at the interior surface gives the supply of the liquid necessary to carry the coloring material to the exterior surface, and deposit it at the outer orifices of the pores.

The cause of the phenomenon being known, a remedy was readily suggested, which consisted in covering the surface of the stone to be embedded in mortar with a coating of asphaltum. This remedy has apparently proved successful. The discoloration is gradually disappearing, and in time will probably be entirely imperceptible.

This marble, with many other specimens, was submitted to the freezing process fifty times in succession. It generally remained in the freezing mixture for twenty-four hours, but sometimes was frozen twice in the same day. The quantity of material lost was .00315 parts of an ounce. On these data Captain Meigs has founded an interesting calculation, which consists in determining the depth to which the exfoliation extended below the surface as the effect of its having been frozen fifty times. He found this to be very nearly the ten-thousandth part of an inch. Now, if we allow the alternations of freezing and thawing in a year on an average to be fifty times each, which, in this latitude, would be a liberal one, it would require ten thousand years for the surface of the marble to be exfoliated to the depth of one inch. This fact may be interesting to the geologist as well as the builder.

Quite a number of different varieties of marble were experimented upon. A full statement of the result of each will be given in the reports of the committees.

At the meeting of the Association at Cleveland, I made a communication on the subject of *cohesion*. The paper, however, was presented at the last hour; the facts were not fully stated, and have never been published. I will, therefore, occupy your time in briefly presenting some of the facts I then intended to communicate, and which I have since verified by further experiments and observations.

In a series of experiments made some ten years ago, I showed that the attraction for each other of the particles of a substance in a liquid form was as great as that of the same substance in a solid form. Consequently, the distinction between liquidity and solidity does not consist in a difference in the attractive power occasioned directly by he repulsion of heat; but it depends upon the perfect mobility of the atoms, or a lateral cohesion. We may explain this by assuming an incipient crystallization of atoms into molecules, and consider the first effect of heat as that of breaking down these crystals, and permitting each atom to move freely around every other. When this crystalline arrangement is perfect, and no lateral motion is allowed in the atoms, the body may be denominated perfectly rigid. We have approximately an example of this in cast-steel, in which no slipping takes place of the parts on each other, or no material elongation of the mass; and when a rupture is produced by a tensile force, a rod of this material is broken with a transverse fracture of the same size as that of the original section of the bar. In this case every atom is separated at once from the other, and the breaking weight may be considered as a measure of the attraction of cohesion of the atoms of the metal.

The effect, however, is quite different when we attempt to pull apart a rod of lead. The atoms or molecules slip upon each other. The rod is increased in length, and diminished in thickness, until a separation is produced. Instead of lead, we may use still softer materials, such as wax, putty, &c., until at length we arrive at a substance in a liquid form. This will stand at the extremity of the scale, and between extreme rigidity on the one hand, and extreme liquidity on the other, we may find a series of substances gradually shading from one extremity to the other.

According to the views I have presented, the difference in the tenacity in steel and lead does not consist in the attractive cohesion of the atoms, but in the capability of slipping upon each other. From this view, it follows that the form of the material ought to have some effect upon its tenacity, and also that the strength of the article should depend in some degree upon the process to which it had been subjected.

For example, I have found that softer substances, in which the outer atoms have freedom of motion, while the inner ones by the pressure of those exterior are more confined, break unequally; the inner fibres, if I may so call the rows of atoms, give way first, and entirely separate, while the exterior fibres show but little indications of a change of this kind.

If a cylindrical rod of lead three quarters of an inch in diameter be turned down on a lathe in one part to about half an inch, and then be gradually broken by a force exerted in the direction of its length, it will exhibit a cylindrical hollow along its axis of half an inch in length, and at least a tenth of an inch in diameter. With substances of greater rigidity this effect is less apparent, but it exists even in iron, and the interior fibres of a rod of this metal may be entirely separated, while the outer surface presents no appearance of change.

From this it would appear that metals should never be elongated by mere stretching, but in all cases by the process of wire-drawing, or rolling. A wire or bar must always be weakened by a force which permanently increases its length without at the same time compressing it.

Another effect of the lateral motion of the atoms of a soft heavy body, when acted upon by a percussive force with a hammer of small dimensions in comparison with the mass of metal, — for example, if a large shaft of iron be hammered with an ordinary sledge, — is a tendency to expand the surface so as to make it separate from the middle portions. The interior of the mass by its own inertia becomes as it were an anvil, between which and the hammer the exterior portions are stretched longitudinally and transversely. I here exhibit to the Association a piece of iron originally from a square bar four feet long, which has been so hammered as to produce a perforation of the whole length entirely through the axis. The bar could be seen through, as if it were the tube of a telescope.

This fact appears to me to be of great importance in a practical point of view, and may be connected with many of the lamentable accidents which have occurred in the breaking of the axles of locomotive engines. These, in all cases, ought to be formed by *rolling*, and not with the hammer.

The whole subject of the molecular constitution of matter offers a rich field for investigation, and isolated facts, which are familiar to almost every one when attentively studied, will be made to yield results alike interesting to abstract science and practical art.

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4. On the Effect of mingling Radiating Substances with Combustible Materials. By Professor Joseph Henry, of Washington.

I beg leave to call the attention of the Association for a few moments to a paper published by our distinguished countryman, Count Rumford, in 1802, in the first volume of the Journal of the Royal Institution of Great Britain, page 28, entitled, "Observations relative to the Means of Increasing the Quantities of Heat obtained in the Combustion of Fuel."

"It is a fact," says Count Rumford, "which has long been known, that clay and several other incombustible substances, when mixed with sea-coal in certain proportions, cause the latter to give out more heat in its combustion than it can be made to produce when it is burnt pure or unmixed."

"It has been ascertained that when the sides and back of an open chimney fireplace, in which coals are burnt, are composed of fire-brick and heated red-hot, they throw off into the room more heat than the burning coals themselves."

"The fuel, therefore," says Count Rumford, "should be disposed or placed so as to heat the back and sides of the grate, which must always be constructed of fire-brick, and never of iron."

The vertical stratum of coal should be as thin as is consistent with perfect combustion, for a large mass of coal in the grates arrests the rays which proceed from the back and sides of the grate, and prevents their coming into the room. The grate or fireplace itself may be so contrived as to produce a proper degree of radiation, but when this is not the case, Rumford advises that the bottom of the grate be covered with a single layer of balls of fire-brick, each perfectly globular, and about two inches and a half or two and three quarters in diameter. "On this layer of balls fire is to be kindled, and in filling the grate, more balls are to be added with the coals, care being taken to mix the coals and balls well together in due proportions. If this is done, the fire will not only be very beautiful, but will send off a much greater quantity of radiant heat into the room than without them." Rumford also declares, that these balls cause the cinders to be almost entirely consumed. "The same effect is said to be produced by the mixture of coals and clay when the fuel is burnt in a close fireplace, such as an iron stove; and it is the custom in the Netherlands to mix moistened clay with the coals before they are introduced into a stove of this form."

Count Rumford gives no account, in the paper I have cited, of experiments by which the fact of the greater radiation from the balls was tested.

In reading his paper some years since, the idea occurred to me that this experiment would be worthy of repetition, with the more manageable and delicate appliances which science has of late years furnished for the use of the investigator. For this purpose I employed the thermo-electrical apparatus of Melloni, furnished with a tube like a telescope to circumscribe the field of radiation, and the result confirmed the statement of Rumford, that more heat was radiated from pieces of fire-brick mingled with the coal than from the combustible itself. The effect, however, would probably have been greater with bituminous coal. The arrangement for experimenting with coal in a fireplace was very imperfect, and I had recourse to the heat produced by the flame of a spirit-lamp, and also of a jet of hydrogen. A flame of this kind was placed before the thermo-pile, at such a distance that the needle of the galvanometer stood at 15°; the end of a platinum wire coiled into a spiral form was then introduced into the flame, and an instant increase of the radiation of heat was observed, the galvanometer advancing to 27°.

It has long been well known, that the introduction of a platinum wire into a pale flame of this character greatly increases the radiation of light, and from this experiment it is evident that the radiation of heat is increased in a like degree. After this a number of different substances were employed, such as glass, carbonate of lime, sulphate of lime, stone coal, fire clay, &c. The greatest effect appeared to be produced with pieces of carbonate of lime. The exact order, however, could not be determined without procuring a series of balls of the same diameter of these different substances. The most striking effect was produced at the very top of the flame, placing the platinum wire in the heated though almost non-luminous air, immediately above the highest point of combustion.

We cannot suppose in these experiments that the absolute amount of heat produced by the combustion of a given quantity of fuel is increased. The most probable conjecture is, that the heat of combination is converted into radiant heat, and that the flame itself is cooled in proportion as the radiation is increased. In order to bring this idea to the test of experiment, a slip of mica one fifth of an inch in breadth was introduced vertically into the lower part of the flame, while the platinum wire occupied the space just above the top. The slip of mica was placed with its flat side vertically, so as not to affect by its radiation the heat of the wire. With this arrangement the radiation of heat from the platinum was diminished. A corresponding diminution was also produced in the amount of radiant light given off, and this was readily perceptible to the sight. This effect was not due to the cooling of the flame by the conduction of the mica, since it is almost a non-conductor of heat, and this property was exhibited by the fact that the luminosity of the mica was confined to that part which was at the surface of the flame on either side.

It appears, therefore, from these experiments, that the introduction of a solid of great radiating power into a mass of materials in a state of combustion, increases the amount of heat thrown into the space around without increasing the absolute quantity produced by combustion, the increase of radiant heat being at the expense of the heat of combination. To give a practical illustration of the condition of the matter, if a given quantity of fuel is employed in evaporating water, by combustion, under a kettle, the useful effect would be diminished by inserting in the flame beneath or amid the combustible a better radiating substance than itself, while in the case of a fire to warm a room the effect would be directly opposite ; a greater amount of heat would be thrown into the room, and less of the heat of combustion would be carried up the chimney with the escaping gas. Or to give another example. If, over a coal fire, a boiling pot be suspended, and a roasting oven before it, the introduction of a radiating material would increase the effect on the latter at the expense of that on the former.

Count Rumford has elsewhere shown, that flame is a bad conductor of heat, and in stoves and boilers heated by flame it is therefore necessary that the draft be made to impinge with considerable force upon projecting portions of the metal, in order that the greatest amount of heat may be absorbed.

If a column of heated air moves rapidly through a perpendicular stove-pipe, but a comparatively small portion of the heat will be absorbed by the metal and radiated into space around. A cylindrical stratum of non-conducting air in contact with the metal will be comparatively at rest, and through this the moving column of heated air will rapidly ascend, without communicating its heat to the metal. If, however, in this case the current of air be obstructed, and the cylindrical motion deranged by the partial close of a damper, the heat immediately around the point of obstruction will be greatly increased. With a proper arrangement of parts, I have known a dark stove-pipe immediately to become red opposite and above the damper, by the partial closing of this valve. It is probable that heat might be economized in certain cases by introducing radiating materials in flues. It should, however, be recollected, that the draft would be impeded by the introduction of foreign materials;—1st. On account of a direct obstruction; and, 2d. Because of the diminished temperature.

It is frequently stated, in works on chemistry, that the heating power of the flame of the compound blowpipe is very great, while its illuminating power is quite small. The truth is, however, that the radiation of heat from its flame is only commensurate with its radiation of light, and that what tends to increase the one will also increase the other.

The radiation from heated, though non-luminous air, would, from these views, appear to be small, though from meteorological considerations they would seem to be considerable.

That a solid substance increases the radiation of the heat of a flame, is an interesting fact in connection with the nature of heat itself. It would seem to show that the vibrations of gross matter are necessary to give sufficient intensity of impulse to produce the phenomena of ordinary radiant heat. Also, since the light is much increased by the same process, we would infer that, by means of the solid, the vibrations constituting heat are actually converted into those which produce the phenomena of light. The whole subject is worthy of further investigation, both in a practical and abstract scientific point of view.

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5. On our Sense of the Vertical and Horizontal, and on our Perception of Distance. By Lieutenant E. B. Hunt, U. S. Corps of Engineers.

Phenomena are often unobserved, or difficult of analysis, by reason of their connection with our habitual acts of consciousness. Conclusions derived from a lifelong experience of our particular conditions of existence, are practically ranked as intuitions. From infancy onwards, each waking moment's perceptions go to impress these conditions upon us as almost of absolute necessity. Our relations to space and to the earth, in their external or apparent form, are so incessantly subjects of our consciousness, that we come to regard them without special consideration or question. As a mind which has spent its habitual energies in pursuing Analytical Geometry conceives co-ordinate axes, almost as matters of course, in every investigation, so do we all, habitually experiencing the action of gravitation, come to consider its influence, our up and down, our horizon and our general geometrical circumscription, essentially as matters of course, as intuitional perceptions, and as subjects with which reason has little concern. It is doubtless true, however, that we acquire such fundamental ideas from continuous experience, just as positively as we do any other elements of our practical knowledge.

Our ideas of the vertical and horizontal, and our sense of distance, have thus become so firmly established by our lifelong experience, that their origin, antecedent to our philosophizing exercise of reason, has become much obscured. Yet they are clearly capable of rational analysis, and their component elements can be made subjects of experiment and variation. As introductory to some observations of this nature which I have made, I will present such general views as these and other experiences have suggested.

The vertical and horizontal are our habitual co-ordinate axes of external geometrical perception. Always ourselves at the origin of co-ordinates, we refer objects seen to their respective positions, by an instantaneous perceptive location, relative to these axes. We combine a mental estimate of distance with an angular reference to the vertical and horizontal axis and plane. In the horizontal plane, our perceptive usage seems to be to refer to a central line in our field of view by angular co-ordinates. When we attempt to embrace the entire horizon in our mental sweep, we for the most part refer objects to the north and other compass lines. This, however, is a more deliberate process, and our consciousness of its steps is comparatively complete.

In the attempt to define the sources of our perception of the horizontal and vertical, two stages of causation have occurred to me as chiefly concerned in its formation.

The first of these depends on our sensibility in each part of the body and limbs to the action of gravitation, — an obscure physiological sensation of weight and pressure in a particular direction throughout the sensitive components of our bodily frame, — a sensation which is probably greatly dependent on the circulating fluids. A specific difference of sensation notifies us of the precise inclination of each portion of our system, and could we be blindfolded and sustained in any position in the air, without any distinct consciousness of a point of suspension, we should probably still have a clear perception of the vertical at any instant. But in our ordinary position of observation, the body notifies us of the slightest variations of direction in the gravity action on each of its parts, and in the act of running down hill over rough ground, or on points of rock, it is particularly apparent how extremely delicate is this appreciation. In this exercise, our ability to combine the appreciations of ground inequalities, of our forward and downward inertia, of the force of gravity, and of that diminution of effective gravity due to the increasing rapidity of bodily descent, quite suffices to make us wonder. This diminution, as I have repeatedly observed, when starting the rapid run down hill, subtracts much from the force or shock of the footfalls. The skill of jugglers in balancing poles, &c., and that of rope-dancers in self-balancing, are illustrations of the same gravity sensibility. When we consider the unconscious skill of the fingers of the pianist or compositor, and that, in all habitual muscular acts, the tendency is to a mechanical or *quasi* involuntary action, it will not seem singular that our continuous, lifelong perceptions of gravitation in all our sensitive parts should assume an obscure or latent form in our minds, which would cause all its results or effects to seem intuitional. I ascribe that sense of the vertical within the limits of the body, whence we pass to an acquaintance with the same in the external world, entirely to this trained gravity sensibility. In the remarkable case of John Metcalf, who, though blind, was long and successfully employed in locating new roads over Derbyshire Peak, this gravity sensibility must have become particularly acute, as to this only could his skill be fundamentally ascribed.

The second causal stage in our perception of the horizontal and vertical in the external world, will be found to be chiefly visual. The testimony of the body to the direction of gravitation makes us aware of the angle between the vertical and the optic axes, by reason of the permanent organic relation between the body and these axes. In our ordinary or erect position, the mean of this angle, or its value when a perfect muscular equilibrium prevails, is a right angle, and any departure from this mean is indicated by the sensations of the various muscles, which cause the elevation or depression of the optic axes. Thus our sensations alone suffice to indicate to us the vertical and a visual horizon perpendicular to it; and these sensations, checked and verified by a lifelong experience, acquire an accuracy of indication which would hardly be anticipated. To these sensational co-ordinates we spontaneously refer all external objects. There is very much, also, in our ordinary perspectives to suggest and indicate the true horizon. Water, as the broad sheet, the winding river, and the tumbling brook, level bottom-lands and broad prairies, continuous hill-crests and isolated knolls, either directly present horizontal planes, or give such logical indications of them as the experienced eye can readily interpret, so as to derive with much accuracy a true general horizon. Indeed, we seem never to look over an extended view without almost instantly defining, with more or less precision, our supposed horizon. Our previous experience from travelling over ground, doubtless, enters directly in our definite location of this mental horizon. Are we in a valley, we pass our horizon under most of the objects seen. Are we in a middle point of view, we pass it in a mean position. Are we on a hill-top, we pass it tangent to the general sweep of the landscape. Thus experience is made to check and qualify the sensational indications, and we fix our horizon by a compound act of sensation and judgment from past experiences.

In our mental estimate of distance, which we habitually make for all objects seen in perspective, there is doubtless a kindred combination of sensational and reflective elements. Sensation gives us a direct measure of distance for objects near at hand, while for remote objects the element of judgment enters. Our normal vision being binocular, the distance between the optical centres of the two eyes becomes a base line of constant length, to which all external distances are referred with a precision constantly decreasing, both outwards and inwards from the interior limit of distinct vision. The convergence of the optic axes on each external object is a direct result of voluntary muscular action, and its particular value is indicated by a specific sensation in the eyeball muscles. Thus the third dimension of perspective is directly represented by a sensation connected with the perceiving eyes, and adjacent distances are referred, by a real triangulation, to the interocular base line. Being thus sensationally advised of the distances of objects, as far as this base continues to be an appreciable quantity relative to the perspective distances, we have, as it were, a considerable circle around us, within which we perceive, with an accuracy diminishing with distance, the absolute nearness or remoteness of objects. From the distances thus known, we pass on by an act of comparison or judgment to those of more remote objects, where the direct sense of distance fails us. Here, too, as for near objects, we doubtless receive some aid from the conscious focalization of the eye, and from the gradation in the clearness of definition of outlines. All our experience in appreciating distances, gained by travelling over ground and checking our estimates by actual comparison, also involuntarily comes to our aid. Our knowledge of the ordinary or absolute sizes of the various classes of objects seen will, besides, greatly affect our rational estimates of distances. Thus sensation gives a foothold on perspective distances; comparison, or a species of continued secondary triangulation, extends our visual scope; and experience brings in the judgment to correct false estimates. If this view be correct, monocular vision should be much less accurate than binocular, in the estimation of distances near at hand. The lack of the interocular base would make the process of perspective gauging almost exclusively an act of judgment. The only sensational element in the former would be that of a conscious focalization of the eye, which could hardly be of much importance. Thus, I should conclude that persons with only a single effective eye would be bad judges of distances, especially among near objects. How the fact stands, I have had no means of ascertaining.

I will now instance a few observations or experiences, tending to confirm the general views now presented.

1. In passing around a particular curve of the railroad from Worcester to Boston, I have twice observed that the whole landscape on the concave side seemed to dip strongly towards the southeast, while, from the adjoining views and the position relative to the ocean, I am satisfied that the real dip could not be nearly so great as the apparent. This I conceive to result from a composition in the body, of the centrifugal action with gravity, which inclined the sensible vertical from the curve centre, and so apparently lifted the sensible horizon, or depressed the actual ground relative to the same. This instance seems to show clearly the dependence of our horizon on our gravity sensibility.

2. From the summit of Crow Nest, the mountain just north of West Point on the west bank of the Hudson, the view southward presents

two distinct reaches of the river in the same glimpse, which seem entirely separate bodies of water, by reason of a mountain spur, which severs all apparent connection at a distance of some six miles. From the height of over twelve hundred feet, you look down on the river, not more than a mile distant, and can readily fancy this portion an elongated lake embosomed in granite hills. Some fifteen miles below, a second apparent lake is seen, and this, despite your knowledge that it is a lower part of the same river, seems elevated above the water beneath you, by perhaps two hundred and fifty feet. This illusion is due, I conceive, to a dip in the apparent southern horizon, caused by our not appreciating the elevation of our point of view relative to the hill ranges over which we look, and so passing our horizon too low among the South Highland peaks. The boldness of the south face of Crow Nest hides that side from view when on the top, and so makes us think the summit of view lower than it really is. Indeed, from a point some two hundred feet lower on the river face, the height above the river seems greater than from the summit, probably because you there see the whole connecting slope. Thus in passing our visual horizon as a kind of general tangent plane, we give it a south dip which seems to lift the distant water. This illustrates the power of our general visual habit over our gravity sensibility and our distinct knowledge.

3. From Jones's Hill, which forms the west bank of that Sleepy Hollow where the exemplary Ichabod Crane achieved immortal fame, the west view embraces a general slope of three fourths of a mile to the Hudson, then the broad Tappan Sea, and the west bank beyond, half palisades and half highlands. On a quiet afternoon of the last year, I there saw a thick fog so drape the west bank as wholly to hide it from view, yet leaving the east bank and about half of the river in full, clear sight. The fog looked extremely sky-like, and shut down in a clear line on the water. When I simply looked sensationally on this scene, the lower fog-line on the water seemed like a distant water horizon, and all above seemed but a cloudy sky. So strong was this impression, as not to be essentially impaired by seeing, half buried in the fog, the sails of a river sloop, which looked as if resting in the cloud-sky. This illusion seemed to lift the hill on which I stood to a height greatly exceeding its actual elevation. The strong likeness of the fog-boundary to a sea-horizon rotated the apparent horizon so as

to make it dip much below the real one towards the west, thus overcoming both my gravity sensibility and a familiar knowledge of the locality.

4. A few weeks since, in the early morning, when standing on the bow of a sail-boat in Newport harbor, I observed, during a thick fog, a singular apparent distortion of the water surface. The boat seemed to rest in a bowl or hollow of water, some four rods in radius. This bowl seemed bounded by a gently curved swell, which ran tangent to the apparent remote horizon. Thus the water around me seemed some four or five feet lower than the horizon water. This appearance I suppose to have resulted from the fog-line along the water looking like a dim distant horizon, and thus bringing the apparent limiting horizon-circle much too near. This would make the apparent horizon dip below the true, and would thus give a false apparent level to the water as far in as to that point where in looking down on it the true level could be distinctly seen. The true and illusive levels seemed joined by a curved swell, the curve evidently depending on the height of the eye and the density of the fog. This observation, like the preceding, illustrates the power of our idea of a water-horizon.

5. If we observe the moon when a little above the horizon, we can prove, by the simple act of shading with the hand all objects below the moon, that its apparent enlargement is in great part caused by our perception of intervening objects, magnifying our idea of its distance. The disc thus seen isolated over the forefinger seems at once to undergo a striking contraction in apparent size. On dropping the hand, the moon seems again rapidly to enlarge, as intervening objects are seen. This masking of the terrestrial horizon does not, I think, reduce the apparent size quite to that of the meridian moon, but it does not lack much of this result. It may be observed, too, that where we see the sun or moon through a forest, especially when travelling in a railroad car, they seem remarkably dilated, and when we are flitting along in a train they seem to be bounding along a parallel road, some miles off, like monstrous fire-balls. The great number of intervening branches and trunks is obviously the direct cause of this illusion. This reasoning applies directly to explaining the apparently ellipsoidal form of the sky dome. Seeing along the ground many objects on which the binocular or perspective sensibility fastens to elaborate

the sense of distance, the horizontal axes become much elongated in appearance, while the vertical axis, having no intervening objects to aid in its apprehension, is seemingly shortened. That the cause suggested by Euler, or the greater absorption of light by its more oblique transit through the strata of vapor near the horizon, may aid this effect, is highly probable.

6. I was once of a party to observe Saturn through the West Point equatorial, when the several persons observing, on stating their impressions of the planet's apparent size, were found to range in their estimates between the size of an orange and that of a cart-wheel. In this case the lack of all perspective aids makes the visual angle the sole index of size, except perhaps the obscure sense of focalization in the eye, for the vision of instrumental pencils, when not parallel in their final emergence. Hence the utter vagueness of all estimates of apparent size, which must depend on a baseless imagination as to the distance of the object.

7. In sailing on Boston harbor, I have twice seen the phenomena of diverging and converging rays, or what is commonly called "the sun drawing water," both towards the sun in the west and towards the point symmetrically below the horizon in the east. Clear as it rationally was, that these two ray systems were but the opposite perspective views of the same parallel beams of light through the air, I could not by any effort make them seem so sensationally. The opposite ray systems would not blend and look connected. The visual thinning of each beam near the perpendicular made the connection wholly invisible, and so completed the illusory projection of the beams on the sky-dome. Hence, too, an apparent widening of each beam, as it receded from the sun or the opposite point of convergence. This whole appearance is an instance of the complete subjection of the mental to the apparent or pictorial.

8. The decided power of the two eyes to determine visible distances by the convergence of their axes was strikingly shown, in observing from within a room in New York the reflection of a gas-burner globe from the street window, during the decline of daylight. When looking with both eyes, this reflection seemed established firmly just by a branch of a street tree, some ten feet outside the window. By shifting position, this branch seemed to pierce the globe, to be within or without it, all strictly according to distance. But on closing one

eye, I found it easy to transfer the image entirely across the street, by a mere exercise of imagination. When using but one eye, the rigid stability of the binocular image was entirely gone, and the apparent distance seemed almost to become the obedient subject of direct volition.

9. If, in looking over a landscape, we bring the line of the two eyes into a vertical position, by lying down on the ground or otherwise, the scene will be found to undergo some remarkable changes. Hills will seem to recede into the distance, and thus become mountains. The perspective will be found to lose much of its relief, and we shall rather seem to be looking at a picture on a huge panoramic canvas, than on an actual receding perspective. If we observe thus steadily for some time, our sense of the horizon becomes much impaired, and a strange whimsicality of aspect results. In so looking over a sheet of water from three to twenty miles in breadth, I have noticed, not only that all the opposite banks seem about equally remote, but that there was the same apparent hollowing of the near water as mentioned in the case of fog on the water, especially in the line of the nearest opposite land. By bringing both eyes into the same vertical, the interocular base evidently becomes nullified for distance-gauging in the horizontal plane. Hence a loss of discrimination in these distances, and a general averaging in our estimation, which removes the near objects and so causes the apparent hollowing of the water. Objects seem to recede to one general cylinder around you, and the perspective perception is exceedingly impaired. The case thus becomes almost one of monocular vision. The study of these changes in scenery by a simple rotation of our interocular base, is highly interesting and attractive. It shows not only the influence of this base in forming our perspective distribution of distances, but that our sense of the horizon is very dependent for its firmness and precision on our gravity sensibility in the ordinary erect position.

Various other facts illustrative of the general views presented might be instanced, but the above must now suffice.

6. Improvements in the Electric Telegraph, whereby two or more Terminal Stations can make Simultaneous Use of the same Wire. By Moses G. Farmer, of Boston.

It is well known that there are two principal systems of Electric Telegraph in use in this country, commonly known as the "Morse" and "House" systems.

The Morse system communicates intelligence through electro-magnetic apparatus, by the operation of breaking and closing an electric circuit at unequal intervals of time.

The House system, by means of electro-magnetic and other apparatus, transmits information by means of a type or letter wheel, so as to exhibit or print any particular letter at pleasure, which is easily done by appropriate mechanism.

Any number of machines, of either kind, may receive intelligence simultaneously in different parts of an electric circuit, but only one of the machines can be employed at one and the same time in transmitting. A combination of the two systems, however, can be so arranged that the same conductor shall, for all practical purposes, serve for the simultaneous transmission of different messages, as follows.

Suppose that, at right angles to the axis of the type-wheel in the House Telegraph, a slender spring be attached; and suppose the outward extremity of this spring, as it revolves, to come into contact with twenty-eight concentrically arranged and separately insulated metallic segments; and that to each of these segments a wire is attached and connected with one pole of a complete set of the Morse apparatus, while the other pole of the Morse apparatus is in connection with the earth in the usual manner. There will then be twenty-eight Morse instruments successively brought into metallic connection with the axis of the type-wheel by its rotation, and the consequent rotation of the slender spring above mentioned.

Suppose the apparatus above described to be located in Boston, and another set, its exact counterpart, to be placed in New York; the two House machines being connected by a wire in the usual way, and having besides an additional wire supplied with a battery, and connecting the axis of the type-wheel in the Boston machine with the axis of the type-wheel of the machine in New York.

If now the two type-wheels be adjusted together, and made to revolve in the usual manner, whenever the slender springs of the two machines are simultaneously upon their similar segments, say the *A* segments, the Morse machine *a* at Boston will be in metallic connection with the Morse machine *a* in New York, by means of the common wire connecting the axes of the type-wheels, the segments, and the springs in the two machines. Thus by the synchronous revolution of the two type-wheels, the twenty-eight machines at Boston will be successively and independently in connection with the twenty-eight machines in New York once in each revolution; and could there be twenty revolutions of the type-wheels per second, each Morse machine would make almost a continuous line, which could be broken up into groups of shorter and longer lines, or fewer or more dots in a group, the same as is now done with the continuous line in the Morse system. It would perhaps not be advisable or necessary to endeavor to work as many as twenty-eight instruments by means of two wires, but that four at least can be so worked I demonstrated by actual experiment at Boston, on one of the circuits of the Fire Telegraph, June 22d, 1852, in the presence of a number of scientific gentlemen.

Whether, upon a long line of telegraph, the signal which was sent out upon say the *A* segment of one apparatus, might not arrive at the *B* segment of the other apparatus, owing to the rotation of the type-wheels, and the time necessary for the electric wave to pass the wire, is a question which I cannot answer from experiment; it undoubtedly would were the wire sufficiently long, or the rotation of the wheels sufficiently rapid, in reference to the velocity of the wave transmission; and there is no doubt that currents might be sent from the different segments, some in one direction and some in the other, and yet be received at the proper segment at the distant station. I hope to make further experiments with the apparatus shortly, the results of which may throw new light on the phenomena of transmission in the electric wave.

The above sketch embraces the principles of an invention for which letters patent were granted me, March 29th, 1853.

7. On an Index of Papers on Subjects of Mathematical and Physical Science. By Lieutenant E. B. Hunt, U. S. Corps of Engineers.

The history of science exhibits a continual tendency towards specialization. The sphere of each laborer becomes more and more limited as the area of research is expanded and distributed into well-defined specialities. The same thing is observable in scientific research, which, in the mechanical and chemical arts, is familiarized under the name of subdivision of labor. The same advantages and disadvantages of the specializing tendency are equally observable in the domains of science and of manufactures. The restriction of investigation and of industry to limited fields of exercise, has the effect to produce the highest skill within those fields, at the price of narrowness of conception and privation of power concerning all other spheres of action and research. The man whose life is spent in heading pins, becomes almost preternaturally skilful in the manipulations of that manufacture; but this man, in any other sphere of industry, is a blunderer and a bigot. So, too, the man of science who assiduously cultivates a chosen speciality becomes therein pre-eminent, but in so doing he is in great danger of losing his grasp on those generalities which transcend his particular field, and of becoming not only impotent, but bigoted, relative to those branches of research which he has not pursued. As the microscopist restricts his field, but intensifies his vision within that field, so the cultivation of a speciality withdraws into a single study the diffused powers of the original mind. The microscopist and the specialist find the same difficulty in seeing the parts in their relations to the whole.

No problem is more important to the scientific investigator, than that of rightly co-ordinating his general and special culture. If generalization too largely prevail, his life will probably be exuberant, but fruitless. If a speciality absorbs all his powers, there will be abundance of ignoble and innutritious fruit. If he knows how properly to combine the general and the special, he is likely to bear such rich and lasting fruit as has made the names of Newton and Leibnitz, Euler and Lagrange, Cuvier and Agassiz, and all of this illustrious kindred, such unfailing sources of strength. The power to generalize and the power to specialize must coexist in the true magnate of science. Great things may be done by men strong in either, but not the greatest. Whoever neglects general culture in his eager pursuit of the special, harms his own nature. It may be said, that power in specialities can only thus be attained; that specialist devotees are essential to the progress of science; that science, like another Juggernaut, demands the sacrifice of the man himself to his chosen pursuit. Whoever thus reasons has already sacrificed his humanity to his speciality, and is but shouting the praises of Brahma. Nor is it easy to adopt a theory of scientific castes which would make of the specialists a lower and necessary grade, whose business it is to gather materials for the generalizing noble to arrange in their true order around some latent principle which he alone is privileged to see. That some minds are thus noble by nature and by training is certain enough, but it is hard to believe that many who could be of any assistance as specialists cannot also do some part, however small, as the generalizers of their own field and of its relations to others.

The present age of science may with great propriety be called *the monographic age*. We have reached that period when every subdivision of research needs to be presented in the form of a monograph. While it is still possible for the student of any single subdivision to pursue it in all the original papers treating thereon, it is quite impossible for the investigator of a more extended area to study the contained and related subjects in their whole range of original papers. To him it is quite essential that the various subdivisions of research should each be digested, by thorough masters, of their component contributions, and brought out in their orderly, well-balanced proportions. The true, the proven, the mature, has need to be separated by adept monographers from the false, the speculative, and the crude. The contributions of many successive investigators, each tending to place their common speciality on higher ground, and to bring it out in more perfect definition, have all to be digested by one who is qualified to take the judicial point of view, and to use aright the privilege of moderns, who, as Bacon says, are the ancients of knowledge.

The model monograph is one which presents the known, substantive facts of the subject, treated in their natural order and relations, with all attainable clearness, completeness, and brevity; and which gives such an insight into the nature of all original memoirs thereon, as will enable investigators to recur to such originals as may be important for their special purposes. Already the mathematical and physical sciences have hundreds of subjects needing to be monographed, while comparatively few are yet so adequately presented under this form as not to need either a new monograph or a revision of the old. The great number of investigators now at work; the late remarkable increase of periodical publications and memoirs, devoted to scientific subjects; the subdividing or fissiparous tendency of modern science, and its signal proclivity to annex fields of empirical practice and make them rational or strictly scientific; — these and other causes have thrown nearly all the provinces included within the present domains of science into the precise condition to need the services of well-furnished monographers.

An invaluable result of such a presentation of these subjects would be found in the aid which investigators would thus receive towards keeping up their general scientific culture, without turning too much away from their specialities. There seems no possible way by which investigators can maintain the general and the special in correct and harmonious relations, except by means of an ever-increasing expansion of their monographic aids. As the realm of knowledge goes on yearly expanding, and steadily growing more and more to exceed the possible grasp of any one mind, this need of a direct and disembarrassed presentation of all positive results, stated as far as possible in the common language of science, will become continually more pressing. It will be only by the aid of monographs of various degrees of generality that the entire domain of science can henceforth be at all surveyed by any single investigator. The narrowest fields, being first duly digested into monographs, will furnish the materials for monographs of higher generality, and from these, step by step, the ascent will be to that universal monograph which shall draw within its compass all the high generalities of the great epic cosmos in which science will attain its far-off final consummation. When physical science becomes thus organized and digested, we shall no longer be constrained to behold the ardent mind borne away by generalizing speculations beyond all basis of fact, or the far more revolting arid mind cramped within a petty speciality, and disdainfully ignoring all the vast domain of vital generalities, simply because it transcends its own dwarfed and grovelling comprehension. The only worthy type of scientific mind is that which at the same time pursues general and special truth; which can see grand generalities underlying the minutest phenomena, and which can trace in the loftiest generalities the simple convergence of an infinite series of kindred special phenomena. The entire character of the active investigating mind of the coming generation will be much influenced by this monographic tendency and need. The extent to which we shall digest into consistent wholes the now dispersed and fragmentary materials on a long array of subjects in mathematical and physical science, will almost measure the prospective prevalence of that just union of the generalizing and the specializing powers whence alone can spring a true progress in scientific philosophy.

The views now presented are essential preliminaries to a full understanding of the bearings and workings of an index of papers on Mathematical and Physical Science. This subject is one which the naturalists have better appreciated than the mathematicians and physicists. The proof of this will be seen in that laborious work which the genius of Agassiz inaugurated, and which presents, in four octavo volumes, the titles of all known papers on natural history and geology up to its date. These titles are arranged alphabetically under the names of their authors, as is probably most convenient in the departments included. Natural history and geology, being essentially descriptive sciences, are less fitted for the classification of the subjects included than the more abstract or logical mathematical and physical sciences. A logical arrangement by subjects, at least for natural history, will scarcely now be proposed by those interested; though it is possible that Englemann's Bibliography of Natural History, which distributes authors alphabetically under a limited number of general heads, may yet serve as a model for indexing papers or memoirs in the same field.

In proceeding to examine somewhat the subject of an index of all original papers in mathematics and physics, it may be well briefly to state the circumstances which have led me to consider it. As an Assistant in the Coast Survey, I had, on several occasions, to make special investigations in which it was desirable to examine all good relevant authorities and original memoirs. How to do this was the question. To range over series after series, index after index, from beginning to end, would surely bring to light all such papers. But this is a labor of truly appalling magnitude, and not to be thought of for each minor research. It only remained to start with such papers as I chanced to know of, or could find by the few indexes of series at my command. Then, following up all the references which could be gleaned from these sources, I could go on till they were exhausted. By that time any man of moderate patience would himself be exhausted, and indisposed to beat up for further game. Yet what guaranty is there in this process that the very best papers may not entirely escape one's knowledge? I found from experience just this result; nor do I suppose that any person can be sure of having examined all those printed papers, on a given subject of physical science, which are essential to a full comprehension of its history and development. He may find a vast deal more than he likes to read, and yet leave the best of all unknown. This is peculiarly true of those just entering on a career of research. Veterans bear in their memories some traces of the leading papers published during their lifetimes, on all subjects ever likely to enlist their activity. But the neophyte has no such aid, and does not even know how to get at the most accessible memoirs. A great loss of time, in turning over leaves for the want of specific references, is common alike to young and old. Thus, for instance, when I wished to find all the published descriptions of automatic tide-gauges, I spent a great deal of time to find what could be read in five minutes, and am by no means confident that I have seen the best description of the best foreign gauge. Every person's experience must abound in such illustrations.

I did not many times repeat this experience of time sacrifice before seeing the advantage of making this search for materials a general one, and so once for all mustering the forces which were likely to be needed. I was thus led to form the plan of a systematic examination of all the principal series of scientific memoirs and periodicals, for the purpose of extracting such titles of papers as I judged proper for a special index on subjects directly related to the various Coast Survey operations. This project met the hearty approval of the Superintendent, who has doubtless experienced its need more than any other person. He authorized my proceeding with its execution, and has furnished every encouragement. I have already examined considerably over one thousand volumes of memoirs, transactions, scientific periodicals, &c., and it is my expectation to advance this index so far as to permit its being included in the Coast Survey Report of this year as an appendix. It will be impossible for me to exhaust the field, but fortunately a supplement can, at any time, be added, with the contributions of succeeding years, in which omissions from the first index may be supplied. This index will be arranged by means of a detailed classification under subjects, and will embrace various heads of interest to cultivators of physical science at large. Though the principle of selection is that of probable use in the Coast Survey operations, the ground covered will be of considerable extent.

Such a fragment alone, though rather laborious, I should not think worthy to bring before this Association, were it not that it has served to indicate a far more catholic plan, and one which, if executed, will prove a signal benefaction to all cultivators of mathematical and physical science. This plan is simply an extension of the one already defined, so as to arrange in one general repository all titles of papers on mathematical and physical science. The ground unoccupied by the index of Agassiz and his collaborators should all be included, so as to aggregate in the two works references to all scientific researches. Engineering, machines, chemical, mechanical, and artistic technology, would be invaluable additions, and should, if possible, all be embraced. All these titles, being duly extracted, and, when necessary, annotated, would admit of classification into several volumes, each containing a connected group of subjects, so that every investigator would find most of the references he would need in a single volume.

It was to somewhat such a shape that this plan reduced itself, when I came to confer with Professor Henry and Professor Baird of the Smithsonian Institution. In the programme of objects proposed to be accomplished by that Institution is included a project of this very character. Professor Henry, having, as he declares, found particular advantages from using the Mathematical and Physical Index arranged by that clear-minded philosopher, Dr. Thomas Young, on a somewhat similar idea, was not likely to forget this among plans for the increase and diffusion of knowledge. It is indeed a plan for rendering accessible the knowledge already recorded, and as such eminently deserves the aid of that Institution. Were the work done, and well done, the Institution would undoubtedly undertake its publication. But done it is not, and the question is how to accomplish its proper execution. Could I have commanded my own time, the performance of this task would have been a congenial labor. In spite of that precariousness of station which is my professional prerogative, I was much inclined to this undertaking, and probably should have begun it had not the assignment of triple public duties made it simple folly to venture it. Thus, with the best intentions, I am obliged to forego this expectation, and have brought the subject here in the hope that some one, more favored by circumstances, may be induced to undertake so needful an enterprise. I will gladly lend any aid in my power to any one who is both fit and willing thus to work for the good of science. How far assistance and compensation might be allowed by the Smithsonian Institution in the execution of this plan is, of course, not for me to say. From the real value of the proposed work, and the interest felt by the Smithsonian officers in its accomplishment, it is fair to infer that no reasonable aid would be denied which the means of the Institution would authorize. Its valuable collection of memoirs and transactions would be of peculiar value in this connection.

There is one excellent suggestion for which I am indebted to Professor Baird. This is, that an index of American scientific papers would be a useful and proper beginning for such an undertaking. The exceedingly scattered and anomalous vehicles through which American investigations have reached the present time, will make this portion of the search rather peculiar, and it is on this account much more needful. In truth, we do not know the real wealth of our own science, especially those of us who are young in such pursuits. Europe, too, is in a state of deplorable ignorance relative to our investigations; — an ignorance which has considerable excuse too, — for how can we expect foreigners to ferret our science from Patent-Office or Coast-Survey Reports, or other public documents, Regents' reports, State legislative documents, or indeed from any except the standard journals and the volumes of memoirs. Thus there are very good reasons for an American index as preliminary to a general one, with which it could be regularly incorporated.

The work now proposed is certainly one of great labor. It will require several years' time, and an examination of various libraries for its completion. Our own libraries will not offer all the materials needed for its completeness. English, French, German, Dutch, Spanish, Italian, Hungarian, Russian, Swedish, and Latin series will need to be ransacked before the work is finished. A delicate exercise of judgment in accepting or rejecting titles will be required. There will be sundry questions such as these: Shall any anonymous papers be included? Shall papers in the literary magazines be selected? Shall titles be given in full and literally, or cut down, modernized, translated, or annotated? Shall translations be included? Shall reprints into more accessible series be referred to? &c. It has been my practice to include the date in each title, and to give the limiting pages of each memoir as an indication of the fulness with which its subject is treated.

The labor of classification will be one demanding a truly cosmopolite mind. Rightly to distribute the hundreds of subjects, and always to maintain the truly logical catena of succession, will require a broad power of appreciation such as few possess. Many plans of classification might be devised, and each having some advantages. But whatever system is adopted, an alphabetical index of subjects would make it easy of practical use. This study of classification would, of itself, be a most valuable research; for I am confident that any abstract or *a priori* plan would find the materials somewhat refractory, and thus would undergo modification and amplification. The general alphabetical arrangement of titles by author's names would, I think, be quite unfit for this case. The greatest benefit of the whole plan would be in its bringing together references to all which has been written on each subject, and thus at once giving a clew to all the materials for perfect monographs. It is of far less importance in mathematics and physics to know the *who* than the *what*. But both can be sufficiently known by alphabetical arrangement of authors under subjects, and by an index of authors' names with references to all the subjects under which each has titles. Thus all the papers of any author, and all the papers on any subject, may be directly found.

I need not further enlarge on this plan. Should there be any who has capacity and courage to undertake the great enterprise indicated, further hints would be superfluous for him. Should there be several who are willing to associate their labors, each taking certain series, and so together exploring the whole, it will not be difficult to concert a general plan. The greatest difficulty of such an association would be in securing thorough co-operation and uniform execution. The means of classification would of course be by movable slips, and thus most incongruities of plan would be avoided by making the classification the work of one.

When we look at an individual labor so valuable as Poole's Index of Periodical Literature, we cannot doubt that the labor now proposed is destined erelong to enlist far greater industry and talent, and that, if seriously undertaken, it must succeed. Dr. Young's Index, contained in the volumes of his works, would afford valuable aid towards conceiving the plan, though it is very far from perfect, and of too old a date to permit the continuance of its classification without much change. The proposed index seems to be one of those undertakings which the current of events will render too indispensable not to be erelong begun. If so extended as to embrace engineering, machines, and the technology of art and manufactures, (chemistry in all its applications would of course be included,) it will become sufficiently valuable to many merely practical interests to enlist their active support. Our Patent-Office might well afford to defray all the cost of such a work, in those departments over which examinations for patents are required to be made.

If, for a moment, we conceive the result attained, and the entire compass of reference to mathematical and physical papers, brought into a systematic body under specific subject-headings, we shall better realize its value. The course of investigation on any particular subject would be made simple and direct. By yearly supplements, we might be kept informed of new papers beyond our ordinary range. The investigator would proceed to exhaust all the papers of value on any subject in hand, and would know when he was done; thus he would start thoroughly furnished for making additions to existing knowledge, instead of wasting his strength on work already done. The preparation of a monograph would no longer involve a chartless roaming over a boundless sea, but our materials could be used in certainty of their completeness, and in such succession as our convenience might dictate. Monographs thus made easy could not fail to cover field after field with unprecedented completeness and facility. We should in each branch be soon furnished with that clear synoptic presentation of all its important elements and results, which would enable us to give each speciality its true value and relations on the general chart of scientific co-ordination. Our general views would keep pace with our special investigations, and our minds would attain that harmony of culture characteristic of the well-developed man. Alike versed in those grand generalities which form the groundwork of creation and practised in the study of our chosen fields of research, we should steer clear of fruitless speculation and of the bigotry of petty knowledge. Such would be the tendencies of the work proposed. What now remains is simply to do it, and for this nothing is wanting but the man, or men. The benefit is for science among all nations. The benefactor's reward will be a truly honorable distinction, and a consciousness of usefulness such as few living men can rightly claim. When we remember how the mighty dead and the honored living have given record to their best thoughts in hope that the world would not willingly let them die, it becomes in us a deed of pious duty to retrace and render legible the inscriptions on these too neglected record tablets.

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8. Notes on the Wilmington Gunpowder Explosion. By Professor Denison Olmsted, of New Haven.

On the 31st of May, 1854, there occurred at Wilmington, in the State of Delaware, a disastrous explosion of gunpowder. Three wagons from Dupont's mills, which are situated from three to four miles above the town, on the river Brandywine, each laden with one hundred and fifty kegs of powder, weighing in all nearly twelve thousand pounds, were following each other at small intervals, and when in the midst of the town, the powder, by some cause not well understood, was suddenly ignited. The teams with their drivers were instantly destroyed, and desolation and ruin were spread all around, attended by the demolition of a number of buildings, and the loss of several lives.

The *ordinary* effects of such a catastrophe are well known, and I do not purpose to occupy the time of the Association in describing them ; but there were a number of *extraordinary* phenomena attending the explosion, illustrating the energy and peculiar modes of action of pneumatic forces, and possibly helping to explain certain obscure phenomena of tornadoes, which appear to me to be deserving of the attention of men of science.

Not long after the occurrence, with the hope of obtaining a more correct and precise statement of the facts than could be derived from the newspaper accounts of the disaster, I addressed a letter of inquiry to Right Rev. Bishop Lee, whose dwelling-house was situated very near the scene of explosion, and was entirely demolished. The pressing engagements of the Bishop prevented his making the report himself, but he procured it to be done by his son, Mr. Benjamin Lee, whose full and precise statement, now before me, leaves little to be desired in respect to the facts of the case. According to this authority, wagon-loads had been in the habit of following daily the same route for fifty years, without the least accident. So long impunity had produced its natural effect, to render the drivers careless and inattentive to the regulations established by the proprietors, prescribing their speed, and the distance at which they should follow each other. Agreeably to the rules, they had left the mills at intervals of half an hour; yet, on reaching town, the hinder wagons had so increased their speed that all three, on entering the town, were near together, and at the time of the explosion were only twenty-five feet apart. It appeared, therefore, that the hindmost wagon must have travelled at the unusual rate of about six miles an hour; and as parts of the road were rough and gullied, the chance of spilling portions of the powder was greatly increased. Hence some persons endeavored to account for the explosion by supposing that powder spilled from one or more of the wagons was ignited by a spark struck by a horse's hoof. Others ascribed it to the carelessness of the drivers, two of whom were known to have been smoking at the time by the side of their wagons; and others still, to a spark from some neighboring chimney. But as the loads were severally covered closely by canvas drawn over them, I cannot think that either of these hypotheses, or any other that has come to my notice, is based on evidence that is at all conclusive or satisfactory. I will therefore omit, on the present occasion, any inquiry into the cause of the explosion, and limit myself to a few observations on its more remarkable effects.

1. The mechanical forces developed exhibited prodigious *energy*. This, indeed, is no more than every one would expect from the instantaneous explosion of twelve thousand pounds of gunpowder, since, according to Hutton (Tracts, Vol. III. p. 301), the initial force of inflamed gunpowder is nearly two thousand times the pressure of the atmosphere. The immense volume of elastic matter, suddenly evolved from twelve thousand pounds of powder, expanding with an energy so

inconceivable, would displace corresponding portions of the surrounding atmosphere, and impart to it a resistless impulse in all directions. The effects corresponded to such a cause. The drivers and horses were torn limb from limb; houses were demolished; trees uprooted or twisted off; and perfect desolation impressed on surrounding objects. But several special phenomena indicated still more clearly the prodigious energy of the forces developed. A splinter of soft pine, part of a Venetian blind, in the house of Bishop Lee, distant sixty feet from the place of explosion, was driven across the room, and struck point-wise an inch board of the same material, penetrating completely through it, with as clean a cut as a steel-pointed arrow would have made. Fragments of wheel-tire with portions of the heavy oak hubs, scraps of the harness, and a few bolts, were the only remains of the wagons. Flying fragments of the wheel-tire, in many instances, lopped off large limbs of trees, with almost the smoothness left by an edge tool. One heavy piece of tire, about two feet long, was found at the top of a high hill, a quarter of a mile off. Small articles were carried to so great a height, that minute fragments continued to fall for ten or fifteen minutes after the explosion. Glass windows were broken at the distance of more than a mile. But the most astonishing proof of the great mechanical energy developed is now to be stated. Under each wagon was found a large cavity, that beneath the middle wagon being three feet in depth, and ten by five feet in area; and these holes did not appear to be excavations, but simple indentations produced by a downward compressing force. I do not recollect any single fact in the history of gunpowder explosions which more strikingly evinces the mechanical power of the agent than this. What must be the height of that granite column, having a base of fifty square feet, which would by its weight sink a hard surface like that of a constantly travelled street to the depth of five feet? How high a column of mercury would be required to produce such an effect? The force transmitted to water-pipes at the depth of four or five feet was such as to fracture them.

2. The *modus operandi* of the forces was, in many cases, singular and curious. In one of the houses overthrown, the inmates had their clothes torn off. Fragments of the wheel-tires, two feet long and under, were, in every case, either partially or wholly *straightened out*. What was the nature of the force by which such an effect could be

produced? My correspondent remarks, that peculiar effects were produced on *metallic* substances. In every case the shoes were torn from the horses' hoofs. Hinges and bolts were wrenched from doors and shutters, even where they opposed no resistance to the motion of the body to which they belonged. Castors were in many instances detached from heavy pieces of furniture.

3. The effect produced on the *animal system*, in cases where the violence was not sufficient to destroy life, was also in many instances remarkable. At the distance of a quarter of a mile from the place of explosion, men were raised from the ground, and were sometimes borne along for several feet, without being prostrated. A man on horseback, at about half that distance, was raised from the saddle, but settled into it again without injury. Persons in the immediate vicinity experienced a sense of suffocation and difficulty of breathing, followed in some instances by soreness of the throat and a slight hæmoptysis.

4. The pneumatic forces appear to have acted by the direct impulse of the elastic medium, rushing outwards, and sometimes by the inward expansion of confined air acting against a vacuum. Near objects were more commonly affected in the former way, and more remote objects in the latter. Thus the doors and windows of a house near the place of explosion were all *driven in*, while in houses at a greater distance they all fell *outwards;* and the walls of the same house, in some instances, which were on the side next the scene of explosion, fell inwards, while the opposite walls fell outwards. A piano open at the time was very little injured, while one closed, situated at a greater distance, was nearly ruined by bursting from within. Large mirrors were thrown from the mantle to the floor without being broken. But glass windows were broken at the distance of more than a mile.

5. We trace a resemblance between some of the foregoing facts and certain phenomena observed in *tornadoes*. The splinter driven through an inch board reminds me of a fact that occurred in the New Haven tornado of 1839, where a piece of board from a bureau was carried half a mile, and was found sticking in the side of a barn, having penetrated through a thick plank. In the same storm, hinges were torn from doors, as in the case of the explosion, by some mysterious force, which did not seem to exert any great violence on the

doors themselves. This mode of action, also, seems analogous to that by which, as in the explosion, shoes were wrenched from horses' feet, and castors from pieces of furniture. Clothes torn from the persons of individuals at some distance from the scene of explosion, appear somewhat analogous to feathers stripped from domestic fowls, as was the case in the New Haven tornado, and in the Ohio storm of 1842, described by Professor Loomis; and the same has been observed in various other tornadoes. The buoyant force by which persons at some distance were sustained when driven by the force of the explosion, as the rider who was lifted from the saddle and replaced without injury, has also been noticed in storms, when individuals, as well as inanimate objects, have been transported in the air to some distance, and set down upon the ground without violence. In the New Haven tornado, a coach-wheel, which was driven against a barn with such force as to leave its impress on the wall, still fell to the ground so gently as scarcely to indent it. In storms, as well as in the explosion, houses have been burst outwards, like a breaking jar filled with air, under an exhausted receiver. In the New Brunswick storm, desks were broken open, in a manner resembling the bursting outwards of the closed piano, as mentioned in the foregoing statement.

The straightening of the wheel-tire is a fact to which I remember no parallel in the description of storms, and the compression of a hard gravelly surface fifty square feet in area to the depth of five feet, as was asserted to have occurred beneath the middle wagon, was an exhibition of an elastic force to which I have recognized few equals among terrestrial forces.

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9. Account of Experiments on the alleged Spontaneous Separation of Alcohol and Water, made at the Smithsonian Institution. Communicated by Professor Joseph Henry, Secretary of the Smithsonian Institution.

At the last meeting of the American Association, a notice was given of a new process for procuring alcohol, for which a patent had been granted. The weak spirit, left to itself in a vessel of great height, was said to separate spontaneously into a strong alcohol, which rose to the top of the column, and into a weaker spirit which was found at the bottom.

For the following statement and remarks relative to granting the patent, I am indebted to Dr. Gale, one of the principal examiners of the Patent-Office.

" When the alleged invention was presented, much doubt was expressed as to the working of the plan, and the author was requested to answer the following questions to satisfy the office on the subject : —

" ' Have you employed this device for purifying alcohol or whiskey ? If so, please state what kind, what size, and what proportioned apparatus you have used on a working scale, and what results you have obtained.'

" To this the applicant replies : —

" ' I have used this device as a mode of separating alcohol from whiskey for several months. The column was of wrought iron, about one hundred feet high, and twelve inches in diameter. It was elevated from the cellar through and above the building; the whiskey was forced in from the upper room of the building through an iron pipe leading over the top of the column and down the inside about fifty feet. This sized column will, I find, separate about two hundred gallons of alcohol from the water, in the space of twelve hours. The larger the diameter, the more rapid the process of separation.'

" It had been stated by the party in correspondence that he had been led to the trial of the experiment by noticing that the liquor in the upper part of a tall standing cask was thought to be stronger than that drawn out near the bottom."

This statement would seem to receive some countenance from the following remarks on the same subject, in Gmelin's Treatise on Chemistry, Vol. I. p. 112, English edition : —

" Similarly, brandy kept in casks is said to contain a greater proportion of spirit in the upper, and of water in the lower part. Here again the question may be raised, whether the cask may have been filled with successive portions of different strengths, which may have disposed themselves in layers one above another."

" As to the propriety," says Dr. Gale, " of granting or refusing a patent, on the evidence before the office, in consideration of the oath of the inventor, the want of means in the office to satisfactorily verify or disprove the experiment, and, lastly, the subsequent statement of the inventor, that he had verified the experiment by several months' work on a practical scale, these facts were regarded as good ground for issuing the patent. If the party should be found to have made a false statement, and so committed a fraud on the Patent-Office, these acts were his own, and for which he must be held responsible."

If the result said to be obtained were true, it would follow that the affinity of bodies for each other would be modified by pressure. Though, from theoretical considerations, it might not be thought impossible that the attraction of two substances for each other might be increased by an increase of pressure, yet there is no antecedent probability that the attraction would be diminished under this influence. But as an account of this invention had been widely circulated in the newspapers, its author had received from the Patent-Office the right to vend the privilege of its use, and the public were exposed to be defrauded in the purchase of that which was worthless, it seemed desirable to settle the question as to the truth of the principle by direct experiment, irrespective of theoretical considerations, and on a scale of sufficient magnitude to leave no doubt as to the result.

With this view, in behalf of the Smithsonian Institution, I accepted the proffered co-operation of Professor Schaeffer, of the Patent-Office, and directed the putting up of the necessary apparatus in one of the towers of the Smithsonian building. The determination of the density of the liquid, and the details of the experiments, were intrusted to Professor Schaeffer, to whom I am also indebted for the following account of the process employed and the results obtained.

As the successful experiment was said to have been made with a column of liquid nearly one hundred feet high, and as the pressure of such a column was given as the cause of the separation of the water or alcohol from the mixture, the repetition of the experiment should be on a corresponding scale.

The great tower of the Institution building was already fitted for experiments requiring like conveniences. A well, or series of openings giving a height of over one hundred feet, passing through several stories, was the place selected. A series of stout iron tubes of about an inch and a half in internal diameter formed the column, the total length of which was one hundred and six feet. Four stop-cocks were provided; one at bottom, one about four feet from the top, and the other two to divide the interval equally, or nearly so.

The liquor used was common rye whiskey of 44 per cent at 60° Fahr., and of 44 on the United States Revenue Hydrometer, one of which was used in testing the liquor.

The experiment commenced on the 18th of November, 1854; a leak occurring caused the trial to be limited to the lower thirty feet, after the lapse of a few hours. On the 20th, the tube was refilled, and after testing at intervals of a few days, the loss was supplied, the whole apparatus, with each cock and the top sealed up, was left to itself until December 14th, when it was again tried at each cock. With a slightly diminished quantity, about one hundred feet in height, the whole again stood until the 18th of April, 1855, when the tests were again made.

Fortunately for the result, the original liquor had been repeatedly tested at different temperatures; the contents of every vessel used to contain it having been tried at each of the several fillings of the tube, which were made on the first days of the experiment, when a leak required its discharge for the purpose of tightening the joints. A portion of the original liquid which had been set aside was also tried at the end of the experiment, and at different temperatures.

The readings of the hydrometer were made with as much accuracy as possible under the circumstances, some of them being taken late at night, and exposed in the open tower to a violent wind. No pains were spared to test the liquid under every variety of circumstances. At first, the windows of the tower were open, but for the last two or three months they were closed. Fifty-four readings were made; nineteen of which were from the original liquid, and the remainder on that drawn from the different cocks. The result may be stated as follows.

On plotting the readings of indication and temperature, they all follow nearly in the same line, the deviations of those taken from the original fluid being quite as great as those taken from either the bottom or top, even after the lapse of months. Or, in other words, within the limits of error (the extreme being but a portion of a degree of the hydrometer), there is not the slightest indication of any difference of density between the original liquor, and that from the top or bottom of the column, after the lapse of hours, days, weeks, or months. The fluid at the bottom of the tube, it must be remembered, was for five months exposed to the pressure of a column of fluid at least one hundred feet high. This pressure, however, is much within that at which inferior champagne bottles are burst, and if pressure alone could produce such an effect, wine of that kind should have long ere this given instances of it.

As the fact has been taken for granted, and chemists of repute have made use of it, there seems good ground for thus formally refuting an error which, at first sight, would not appear worthy of being dignified by so much notice.

IV. PHYSICS OF THE GLOBE.

1. Approximate Co-tidal Lines of the Pacific Coast of the United States, from Observations in the United States Coast Survey. By Professor A. D. Bache, Superintendent. (Communicated by Authority of the Treasury Department.)

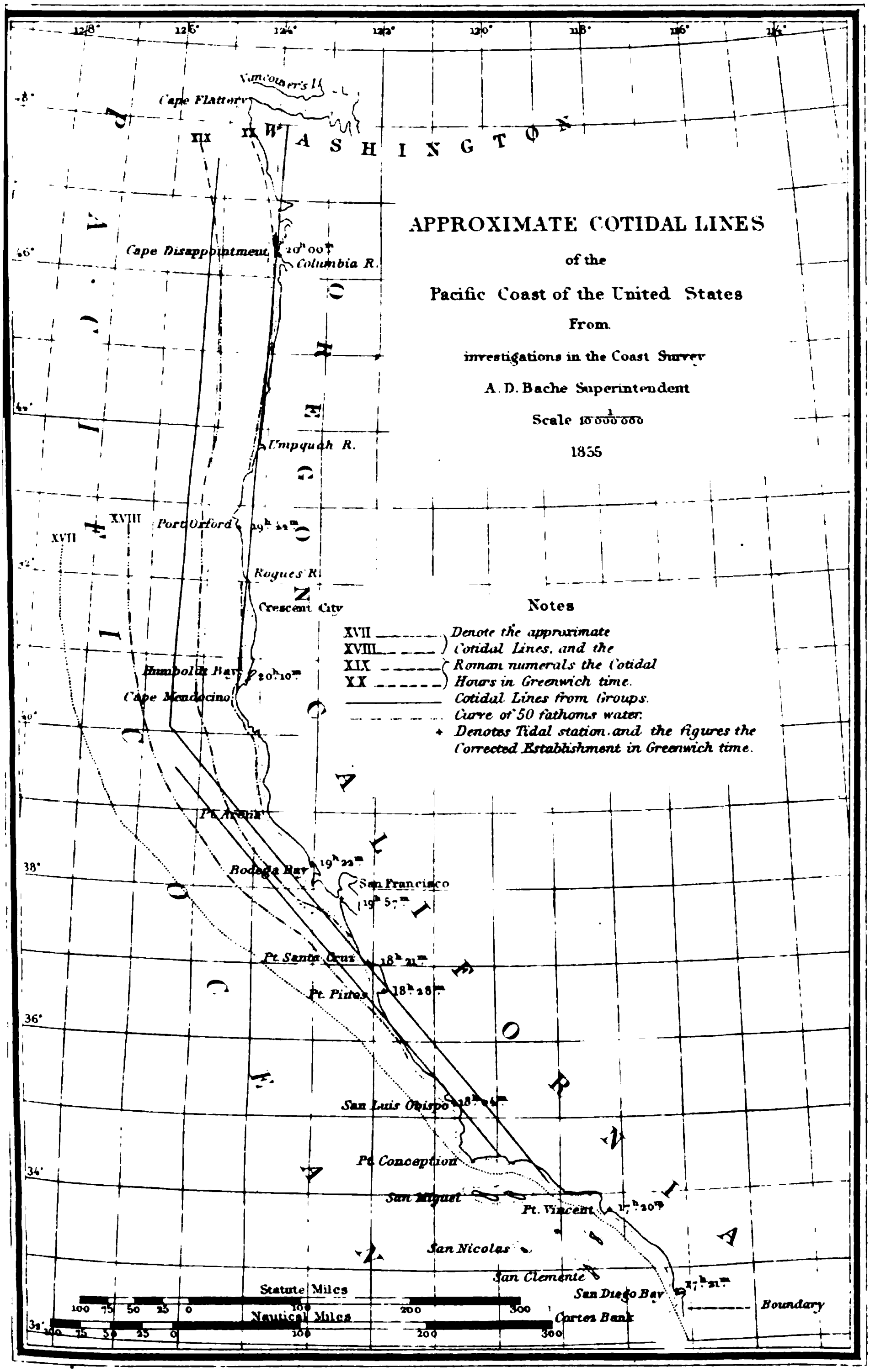
The western coast of the United States, between San Diego, California, and Columbia River, extending through 13° 35′ of latitude and 6° 43′ of longitude, is divided into three reaches (see Plate); the first from San Diego to Point Conception, the second from this point to Cape Mendocino, and the third from that cape to Cape Disappointment at the mouth of the Columbia. The first reach, about two hundred and twenty miles in extent, is curved, the general trend being about N. 56° W. The second, about four hundred and thirty miles in extent, is in general straight, with moderate indentations only, and its trend is about N. 27° W. The third, three hundred and seventy miles in extent, is also nearly straight, trending nearly N. 5° E.

The soundings on the coast generally, except in the harbors, have been for the purpose of general reconnoissance, and are not detailed enough to show the configuration of the bottom.

Tidal Observations.

Tidal stations for long series of observations have been established at San Diego, San Francisco, and Astoria (Columbia River), and, between these, temporary stations at the points and for the periods

P S Duval & Co steam lith. press Phil.



stated in the annexed general table. Saxton's self-registering gauge has been employed at the permanent stations generally, and at some of the temporary stations also.

The observations are under the direction of Lieutenant W. P. Trowbridge, U. S. Corps of Engineers and Assistant in the Coast Survey. They were commenced in 1853, and are still in progress. The very intelligent and careful supervision of this officer is a guaranty for the character of the observations. The observers, too, were especially selected by him for their faithfulness and intelligence.

The number of results collected is such as to warrant an approximate determination of the co-tidal lines of this coast, to be checked when further results are obtained. This attempt has the advantage of pointing out deficiencies in the series which otherwise would not so clearly appear. The following table shows the localities of observation and the duration of each series embraced in this discussion, the name of the observer, and the kind of gauge employed.

TABLE I.

Tide Stations on the Western Coast of the United States, the Results of which are discussed in this Paper.

	Stations.	Time.		Gauges.	Observers.
1	San Diego,	9 months	(1853-4)	Self-registering,	A. Cassidy.
2	San Pedro,	4 "	(1853-4)	"	T. A. Szabo.
3	San Luis Obispo,	2 "	(1854)	Box,	G. Sherman.
4	Monterey,	2 "	(1854)	Self-registering,	J. Ord.
5	Santa Cruz,	1 "	(1853)	Staff.	
6	San Francisco,	12 "	(1853-4)	Self-registering,	G. Sherman, H. E. Uhrlardt.
7	Bodega,	3 "	(1854)	"	T. A. Szabo.
8	Humboldt Bay,	2½ "	(1854)	Box,	J. A. Black.
9	Port Orford,	2½ "	(1854)	Self-registering,	T. A. Szabo.
10	C. Disappointment,	2 "	(1854)	Staff,	J. A. Black.
11	Astoria,	9 "	(1853-4)	Self-registering,	I. Wayne.

These results were in part tabulated by Lieutenant Trowbridge, and in part in the Tidal Division of the Coast Survey Office, under the immediate direction of Assistant L. F. Pourtales. The discussions were made in general by Messrs. Heaton and Hawley, of the same Division.

The times of high water are referred to the next preceding transit of the moon, transit F of Mr. Lubbock's nomenclature, the epoch having been found to correspond to that transit. The mean interval between the time of the moon's transit and the time of high water, or the establishment corrected for half-monthly inequality for each station, is given in the following table. A correction to carry the results to deep water is applied in the way described in my paper on the Co-tidal Lines of the Atlantic Coast of the United States,* giving the establishment used in obtaining the co-tidal hour. The latitude and longitude from Greenwich of each tidal station are given in the table to the nearest minute. The co-tidal hour found from the establishment, corrected for depth and the longitude from Greenwich, is in the last column of the table. It is not necessary to apply a correction for the different transits, as the difference between the greatest and least corrections amounts to but five minutes.

TABLE II.

Data for the Co-tidal Lines of the Pacific Coast of the United States.

	Stations.	Corr. Est.	Correction for Depth.	Final Corr. Est.	Latitude N.	Longitude W.	Co-tidal Hour.
		h. m.		h. m.	° ′	° ′	h. m.
1	San Diego, . .	9 42	10	9 32	32 42	117 13	17 21
2	San Pedro, . .	9 37	10	9 27	33 43	118 16	17 20
3	San Luis Obispo,	10 4	3	10 1	35 11	120 43	18 4
4	Monterey, . . .	10 20	0	10 20	36 36	121 54	18 28
5	Santa Cruz, . .	10 16	3	10 13	36 57	122 0	18 21
6	San Francisco, North Beach,	11 56	9	11 47	37 48	122 26	19 57
	Bodega, . . .	11 19	9	11 10	38 18	123 3	19 22
8	Humboldt Bay, .	12 2	9	11 53	40 45	124 10	20 10
9	Port Orford, . .	11 26	0	11 26	42 44	124 29	19 44
10	Columbia River, Cape Disappointment,	12 0	16	11 44	46 17	123 56	20 0

Co-tidal Hours.

The co-tidal hours thus far obtained between San Diego and Cape Disappointment, Columbia River, are contained between $17^{h.}$ $20^{m.}$ and $20^{h.}$ $10^{m.}$, increasing as a general rule, but with striking exceptional cases and not regularly, in passing northward. The co-tidal hour of $17^{h.}$ $20^{m.}$ characterizes the two stations in the southern reach referred to in the description of the coast. $18^{h.}$, $19^{h.}$, $20^{h.}$ are found on the middle reach, and $20^{h.}$ characterizes the northern.

* Proceedings of the Amer. Assoc. for the Adv. of Science, Washington Meeting, 1854.

Co-tidal Groups.

In discussing these results, I have followed the same course as in the paper on the Co-tidal Lines of the Atlantic, dividing the stations into natural groups, and applying Lloyd's mode of discussion of magnetic lines to them.

The *northern group* of stations, between Cape Disappointment and Cape Mendocino (see Plate), is composed of Cape Disappointment, Port Orford, and Humboldt. The mean co-tidal hour is $19^{h.}\ 58^{m.}$. The mean of the longitudes of the stations is 124° 12′; the mean of the latitudes, 42° 15′. Calling the differences between the mean longitude and the longitude of each station when reduced to nautical miles x, the differences between the latitude of each station and the mean y, the difference between the co-tidal hour at each station and the mean co-tidal hour z, and assuming Σ as the sign of the algebraic sum of the numerical quantities obtained for the co-efficients of the equations furnished by each station, we form and solve the equations

$$M\Sigma x^2 + N\Sigma xy = \Sigma xz;$$
$$N\Sigma xy + M\Sigma y^2 = \Sigma yz.$$

In thc case before us, M gives for the co-efficient of the longitude 1.2, and N for that of latitude —0.006. The tangent of the angle which the co-tidal line makes with the meridian $-\frac{N}{M} = 0.05$ and the angle is 2° 52′. The distance in nautical miles perpendicular to the co-tidal line corresponding to one minute of establishment, or $\sqrt{M^2 + N^2}$, is 1.2 miles, and therefore the progress of the tide-wave in one hour fifty miles.

This is a velocity less than the depth would indicate to be correct, and, from the small differences in the establishments of the stations, this must be an uncertain datum. We shall see, however, that in the next group, where the establishment varies more considerably, this datum is still less probable than the one here obtained.

The direction of the line is nearly coincident with that of the trend of the coast, the co-tidal angle being 2° 52′, and the general trend of the coast differing but two degrees from it.

The co-tidal hours calculated from the separate equations are, for Cape Disappointment, $20^{h.}\ 0^{m.}$, for Port Orford, $19^{h.}\ 44^{m.}$, agreeing precisely with the observed, and for Humboldt, $20^{h.}\ 9^{m.}$, differing but one minute from the observed.

The observations bearing upon this group are extending northward, but the difficulties in the way of maintaining the stations are such, on a coast inhabited by aborigines, that I do not venture to count upon speedy results. Lieutenant Trowbridge is using his best efforts to establish the necessary stations.

I precede the discussion of the *middle group* of stations by the table on page 149, which gives the results corresponding to several different hypotheses, which will, in turn, be examined.

Taking the five stations between Cape Mendocino and Point Conception as one group, we find from the table the angle of the co-tidal line with the meridian, N. 35° 30′ W., and the mean co-tidal hour, $18^{h.}\ 50^{m.}$; the difference of establishment for one geographical mile perpendicular to the co-tidal line, 4.7 minutes. As the observations at Santa Cruz are comparatively few in number, it may be more proper to leave out that station, which will give for the corresponding results to those just stated, N. 36° 43′ W. for the angle of the co-tidal line, $18^{h.}\ 58^{m.}$ for the mean co-tidal hour, and 3.9 minutes for the co-tidal difference in one geographical mile.

Omitting Bodega from this group, we obtain for the co-tidal angle, N. 37° 26′ W.; for the mean co-tidal hour, $18^{h.}\ 50^{m.}$; and for the change of hour in one mile, 3.9 minutes.

Omitting Bodega and San Francisco from the first group, the three southern stations, San Luis Obispo, Monterey, and Santa Cruz, give, for the same values, N. 33° 6′ W., $18^{h.}\ 18^{m.}$, and 4.9 minutes. The direction of the co-tidal line being nearly the same, its denomination only is changed. The $18\frac{1}{4}$ hours would give nearly $18\frac{3}{4}$ if carried to the co-tidal line of the first hypothesis, $18^{h.}\ 50^{m.}$, which is a good agreement.

Omissions at the other end of the group produce the same result. Leaving out San Luis Obispo from 1, we obtain for the co-tidal angle, N. 36° 30′ W.; co-tidal hour, $18^{h.}\ 47^{m.}$; change per mile, 4.4 minutes. The same result is obtained by other omissions in the series.

The introduction of Humboldt into a group with Bodega and San Francisco gives results materially different from those obtained, reducing the co-tidal angle to 18° 5′, and increasing the velocity to forty miles per hour.

TABLE III.

Discussion of the Middle Group of Tidal Stations between Cape Mendocino and Point Conception.

	Stations.	Mean Longitude.	Mean Latitude.	Mean Co-tidal Hour.	M — Diff. of Co-tidal Hour for one Geogr. Mile of Longitude.	N — Diff. of Co-tidal Hour for one Geogr. Mile of Latitude.	Angle [tang = M/N] Co-tidal Angle.	$\sqrt{M^2+N^2}$ Diff. of Co-tidal Hour corresponding to one Geographical Mile perpendicular to Co-tidal Line.	Miles per Hour, Tidal Wave.	Observed — Computed Co-tidal Hour for San Luis Obispo.	Observed — Computed Co-tidal Hour for Monterey.	Observed — Computed Co-tidal Hour for Santa Cruz.	Observed — Computed Co-tidal Hour for San Francisco.	Observed — Computed Co-tidal Hour for Bodega.
		° ′	° ′	h. m.			° ′			m.	m.	m.	m.	m.
1	San Luis Obispo, Monterey, Santa Cruz, San Francisco, Bodega,	122 1	36 58	18 50	3.83	2.73	35 30	4.7	13	7	12	—30	12	4
2	San Luis Obispo, Monterey, San Francisco, Bodega,	122 2	36 58	18 58	3.11	2.32	36 43	3.9	15	1	1		2	8
3	San Luis Obispo, Monterey, San Francisco,	121 41	36 32	18 50	3.07	2.35	37 26	3.9	15					
4	San Luis Obispo, Monterey, Santa Cruz,	121 32	36 15	18 18	4.1	2.7	33 6	4.86	13					
5	Monterey, Santa Cruz, San Francisco, Bodega,	122 21	37 25	18 47	3.5	2.6	36 30	4.42	14					
6	Monterey, San Francisco, Bodega,	122 28	37 34	19 16	3.35	2.6	37 37	4.2	14		0		—1	—2

The combination of San Pedro with the southern stations also changes the result so rapidly, as to prove that the group is limited to the south of Point Conception.

The proof seems complete, that these five stations form a single group. Using the determination in which Santa Cruz is omitted, for reasons already stated, we have for the co-tidal angle N. 36° 43′ W., which gives an inclination to the general line of the coast of about ten degrees. The line of nineteen hours meets the coast north of Point Año Nuevo, and between it and Point San Pedro.

The comparison of the observed and computed establishments from either of these hypotheses is very satisfactory ; — from that of the five stations, Santa Cruz alone stands out with a difference greater than fifteen minutes. For the second list of four stations, the greatest difference is twelve minutes, and the mean, without regard to signs, is but six minutes.

The velocity of the tide-wave is less satisfactory from the other data, rising to but fifteen miles per hour. The depth should give a greater velocity, and the comparison with the northern group would indicate a much greater.

In drawing the chart of co-tidal lines I have not followed the velocities strictly. This group, however, lies favorably for the determination of the rate of motion of the tide-wave, and the results of the various hypotheses in the table are quite consistent with each other in giving a low velocity.

The *southern group* is imperfect, as having but two stations in it. Further observations are required here, and on the islands which separate Santa Barbara Sound from the great ocean.

Combining San Luis Obispo with San Diego and San Pedro would require a retrograde wave, showing that they do not belong to the same group.

The computations required in these discussions were generally made by Mr. Heaton of the Tidal Division, under my immediate direction or that of Assistant Pourtales.

Chart of Co-tidal Lines.

From this discussion I have drawn a chart of approximate co-tidal lines for the coast of Oregon and California. (See Plate.)

The chart, on a scale of $\frac{1}{10,000,000}$, the same which was used in presenting the co-tidal lines of the Atlantic coast of the United States, shows the general configuration of the coast.

The co-tidal hours are marked near the several tidal stations.

The straight lines resulting from the discussion of the northern and middle groups are delineated; for the northern group, the co-tidal lines of XIX. and XX. hours, and for the middle group, of XVII., XVIII., XIX., and XX. hours.

The curves representing the approximate co-tidal lines of XVII., XVIII., XIX., and XX. hours are drawn in dotted lines, the character of the dots differing for the several lines.

The line of XVII. hours 20$^{m.}$ would follow the coast nearly from San Diego to Point Conception, then the line of XVIII. hours nearly to Point Pinos. North of this point, the lines of XVIII. and XIX. hours meet the coast obliquely at an angle of about ten degrees, the line of XX. hours appearing near Point Arena, and following the coast generally to Cape Disappointment, the receding parts having a little later, and the projecting parts a little earlier hour.

Throughout the extent of coast examined, the co-tidal lines are either sensibly parallel to, or make a small angle with, the general direction of the coast. The angle made with the coast between Point Conception and Cape Mendocino is greater than is general on the long reaches of the Atlantic coast.

The successive charts of co-tidal lines of the Pacific have been tending towards the representation now given, as more reliable observations have been collected.

The last chart, in 1848, of the Master of Trinity (Rev. W. Whewell),* to whom this subject owes so much of its progress, in comparison with that of Rear Admiral Lutke,† or with his own earlier map,‡ shows this tendency, the inclination of the lines to the coast being lessened at each step.

* Royal Society's Transactions, Vol. LXVI., 1848.

† Bulletin de la Classe Physico-Mathematique de l'Acad. Imp. des Sciences de Petersbourg, Tom. II. No. 1.

‡ Royal Society's Transactions, Vol. LI., 1833.

2. Notice of the Tidal Observations made on the Coast of the United States on the Gulf of Mexico, with Type Curves at the several Stations, and their Decomposition into the Curves of Diurnal and Semi-Diurnal Tides. By A. D. Bache, Superintendent U. S. Coast Survey. (Communicated under authority of the Treasury Department.)

Abstract.

The stations are eighteen in number; at four, hourly observations were made for one year or more; and at the remainder, for not less than two lunations, and generally for more. The stations at Cape Florida, Indian Key, Key West, and Tortugas, were intended to trace the tide-wave through the Florida Channel; those at Egmont Key (Tampa), Cedar Keys, and St. Mark's, to trace it along the western coast of Florida; at St. George's, Pensacola, Fort Morgan, Cat Island, and E. Bayou (entrance to the Mississippi), to trace it along the south coast of Florida, Alabama, Mississippi, and part of Louisiana; at E. Bayou, Dernière Isle, Calcasieu, Bolivar Point and Galveston, Aransas and Brazos Santiago, for the coast of Louisiana and Texas.

The observations were chiefly made by Mr. Gustavus Würdemann, with different assistants. At a few stations they were made by Corporal Thompson of the Engineers, Mr. Bassett, Mr. Tansill, and Mr. Muhr. The reductions were made in the Tidal Division of the Coast Survey Office by Assistant Pourtales, Mr. Gordon, Mr. Mitchell, Mr. Heaton, and others. The methods used were those pointed out in my previous papers to the Association, the decompositions being in some cases made graphically, and at a part of the stations, where the semidiurnal wave is considerable, the ordinary method of working was employed, as well as those considered peculiarly applicable to these tides.

As it would be tedious to present the results of these elaborate discussions in detail, when the co-tidal lines are introduced, I have thought it best briefly to refer now to the types of the different tides, and to present to the Association the diagrams for the several stations, showing upon a uniform scale the normal curves and their decompositions into the diurnal and semi-diurnal waves.

3. NOTICE OF EARTHQUAKE WAVES ON THE WESTERN COAST OF THE UNITED STATES, ON THE 23D AND 25TH OF DECEMBER, 1854. (Communicated by A. D. BACHE, Superintendent U. S. Coast Survey, under authority of the Treasury Department.)

IN February, 1855, I received from Lieut. W. P. Trowbridge of the Corps of Engineers, Assistant in the Coast Survey, in charge of the tidal observations on the Pacific coast, a letter calling my attention to the singular curves traced by the self-registering tide-gauge at San Diego on the 23d and 25th of December, and remarking that the irregularities of the curve could not be produced by disturbances from storms, as the meteorological records for the whole coast showed a continuance at that time of an ordinary state of weather, and the length of the wave was too great to be explained by such action. "There is every reason to presume," he continues, "that the effect was caused by a submarine earthquake." No shock, however, had been felt at San Francisco.

When the record sheet of the self-registering gauge at San Francisco was received, similar irregularities in the curves for the same days were found upon it. The sheet for Astoria presented little or no special irregularity. These were the only self-registering gauges actually in operation at this time.

Waves of short period would, of course, escape detection by the ordinary hourly or half-hourly observations.

About the 20th of June, we received accounts from Japan of a violent earthquake on the 23d of December, the notice of which was more circumstantial than usual from the damage to the Russian frigate *Diana* in the port of Simoda, on the island of Niphon, from the excessive and rapid rise and fall of the water.

A detailed account of the phenomena of this earthquake, and of the rise and fall of the sea produced by it in different places on the coasts of the Pacific, is much to be desired, and I have thought that, by the publication of the results obtained by the Coast Survey, the publication of official reports of the phenomena might be induced. Perhaps even similar observations may have been made, and these registers of the self-acting tide-gauge will show what observations it is desirable to have for comparison.

Thus far we are left to the public prints for the information obtained,* and the different accounts are quite discrepant where they give details, and are usually, as intended merely for general information, too vague in the statements to give satisfactory means of comparison.

A correspondent of the New York Herald writing from Shanghae gives the following notes, stated to be derived from an officer of the frigate *Diana*: —

" At 9 A. M. on the 23d of December, weather clear, thermometer 72°, barometer $30^{in.}$, a severe shock of an earthquake was felt on board the frigate, shaking the ship most severely. This shock lasted full five minutes, and was followed at quick intervals by rapid and severe shocks for thirty minutes.

" At $9^{h.}$ $30^{m.}$ A. M. the sea was observed washing into the bay in one immense wave, thirty feet high, with awful velocity. In an instant the town of Simoda was overwhelmed, and swept from its foundations. . . . . .

" This advance and recession of the water occurred five times. . . . . .

" By $2^{h.}$ $30^{m.}$ P. M. all was quiet."

A communication in the same paper, purporting to give an extract from the log-book of the *Diana*, states that, —

" At a quarter past nine, without any previous indication, the shock of an earthquake, which lasted two or three minutes, causing the vessel to shake very much, was felt both on deck and in the cabin.

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* Since reading this paper, I have received, through the kindness of Commodore M. C. Perry, a copy of a letter from Captain H. A. Adams, U. S. N., who visited Japan in the steamer *Powhatan*, to exchange ratifications of the treaty between Japan and the United States. Captain Adams says: " Simoda has suffered dreadfully since your visit there. On the 23d of December there were several shocks of earthquake. The sea rose in a wave five fathoms above its usual height, overflowing the town, and carrying houses and temples before it in its retreat. When it fell, it left but four feet of water in the harbor. It rose and sunk this way five or six times, covering the shores of the bay with the wrecks of boats, junks, and buildings. Only sixteen houses were left standing in the whole place. The entire coast of Japan seems to have suffered by this calamity. Yedo itself was injured, and the fine city of Osaka entirely destroyed."

Captain Adams then gives an account of the disaster to the Russian friagate *Diana*, Admiral Pontiatine commanding, which was so injured in the harbor of Simoda as to lead finally to her entire loss.

At ten o'clock a large wave was observed entering the bay. . . . . . The rising and falling of the water were very great, the depth varying from less than eight to more than forty feet; and these changes, at intervals of about five minutes, continued until noon. . . . . . Scarcely had half an hour elapsed, when the rising and falling of the water became more violent than before. Between this time and a quarter past two (when the agitation again became much less) the frigate was left four times on her side, and once while thus laid, in only four feet of water. . . . . .

"Continuing to decrease in violence and frequency by 3 P. M., the agitation of the water, and the motion of the vessel consequent thereon, were very slow. . . . . . At this time a fresh west wind was blowing, the barometer stood at $29^{in.}.87$, and the thermometer was 10.5 degrees R. (about 55.6 degrees F.)"

The official report of the disaster to the frigate will probably contain further and more precise particulars of the phenomena.

Mr. P. W. Graves gives, in the Polynesian, a notice, for which I am indebted to Mr. Meriam, of an extraordinary rise and fall in the waters at Peel's Island, one of the Bonin Islands, on the 23d of December. The first rise noticed was fifteen feet above high water, followed by a fall which left the reefs entirely bare. The hour when this occurred is not stated. "The tide continued to rise and fall during the day, at intervals of fifteen minutes, gradually lessening" until the evening.

At Peel's Island the waters rose on the evening of the 25th of December to the height of twelve feet. I have not, however, seen any notice of an earthquake on that day.

I present to the Association a copy of the curves traced by the self-registering gauges at the Coast Survey tidal stations at San Diego, San Francisco, and Astoria, on the 23d and 25th of December, 1854. (See Plate.)

The curves representing tides of short period being traced upon the falling or rising curve of the regular tide, their peculiarities are not so readily seen as when shown in the second diagram (see Plate), where the regular tidal curve is represented as a horizontal line. The times of the San Diego curve are reduced to San Francisco time. The curve at San Diego presents many minor irregularities, from the motion of the float not having been sufficiently checked to prevent the recording of the waves caused by the wind.

Upon a falling tide, the crests of these waves will be met earlier, and the hollows later, than upon a horizontal surface, and the intervals from crest to crest, or from hollow to hollow, will be affected by the change of rate of fall. Upon a rising tide, the reverse will occur.

There can be no doubt that these extraordinary rises and falls of the water at short intervals, were produced by the same cause which determined the extraordinary rise and fall in the harbor of Simoda in Japan, and at Peel's Island.

The *San Francisco* curve presents three sets of waves of short interval. The first begins at about $4^{h.}$ $12^{m.}$, and ends at $8^{h.}$ $52^{m.}$, the interval being $4^{h.}$ $40^{m.}$ The second begins at about $9^{h.}$ $35^{m.}$, and ends at $13^{h.}$ $45^{m.}$, the interval being $4^{h.}$ $10^{m.}$ The beginning of the third is about $13\frac{3}{4}$ hrs., and its end is not distinctly traceable.

The crest of the first large wave of the three sets occurred at the respective times of $4^{h.}$ $42^{m.}$, $9^{h.}$ $54^{m.}$, and $14^{h.}$ $17^{m.}$, giving intervals of $5^{h.}$ $12^{m.}$ and $4^{h.}$ $23^{m.}$

The average time of oscillation of one of the first set of waves was $35^{m.}$, of one of the second $31^{m.}$, and of one of the third about the same. The average height of the first set of waves was .45 of a foot on a tide which fell two feet, of the second .19 of a foot on a tide which rose three feet, of the third somewhat less than .10 of a foot on a tide which fell some seven feet. The phenomena occurred on a day when the diurnal inequality of the tide was very considerable. The greatest fall of the tide during the occurrence of the first set of waves was .70 ft., and the corresponding rise .60 ft. In the second, the corresponding quantities were .30 ft., and in the third, .20 ft. These waves would not have attracted general attention.

There is a general analogy in the sequence of the waves of the three sets, which seems to mark them as belonging to a recurrence of the same series of phenomena. In the diagram No. 3, A (see Plate), the heights of the successive waves of the first set at San Francisco are shown by the dots joined by full lines, and of the second, by those joined by the fine dotted lines. The full lines show the heights of the first series at San Diego, and the broken lines the heights of the second. The heights in hundredths of a foot are marked at the side of the diagram, and those of the successive waves are placed at regular intervals, the waves being numbered from 0 to 7 at the top of the diagram. The height is the mean of the fall from a crest to a hollow, and of the succeeding rise from the same hollow to the next crest. The times of oscillation from one crest to the next succeeding are placed on the same diagram, the times being written at the right hand, and the wave being designated at the lower part of the diagram No. 3, B. The full line represents the times of the first series at San Francisco, and the broken line the times of the second. The full and broken faint lines represent the times of the first and second series at San Diego. The intervals between the times of occurrence of the crests of the successive waves in the first and second series diminish from $5^{h.}$ $10^{m.}$ to $4^{h.}$ $48^{m.}$ by irregular differences.

The effect of the rising or falling tide upon which these waves occur is of course greater in disturbing the heights than the times.

The series itself looks like the result of several impulses, not of a single one, the heights rapidly increasing to the third wave, then diminishing as if the impulse had ceased, then being renewed, then ceasing, leaving the oscillation to extinguish itself.

If we had a good scientific report of the facts as they occurred at Simoda, the subject would lose the conjectural character which must otherwise belong to it. Although we have no account of the place where the earthquake had its origin, the violence of its effects in Japan, and the diminished effects at Peel's Island, show that Japan was certainly not far from the seat of action.

Five successive waves of considerable height are spoken of as having occurred at Simoda, while by the gauge we trace eight, of which seven are of considerable height. The highest wave at Simoda was estimated at thirty feet; at Peel's Island, fifteen feet; — at San Francisco it was 0.65 ft., and at San Diego in the first series, 0.50 ft.

At *San Diego* the same three series of waves are distinctly shown. The first begins $1^{h.}$ $22^{m.}$ later than at San Francisco, correction having been made for the difference of longitude, and ends $0^{h.}$ $52^{m.}$ later. The interval is $30^{m.}$ less than at San Francisco, the oscillations being rather shorter than at the last-named point. The second begins at $0^{h.}$ $54^{m.}$ later than at San Francisco, and ends $34^{m.}$ later. The third begins about $54^{m.}$ later than at San Francisco. The average time of oscillation of the first set of waves is $31^{m.}$, and of the second $29^{m.}$, being respectively $4^{m.}$ and $2^{m.}$ less than of the corresponding series at San Francisco.

The average height of the first set of waves was 0.17 ft. lower than at San Francisco, and of the second as much higher. This fact, taken with the difference in the times of oscillation, leads me to suppose the difference in the two series due to interference, which is also suggested by the position of San Diego in reference to the islands separating the Santa Barbara Sound from the ocean.

The general analogy in the succession of heights of the mean of the two series, as shown in diagram No. 3, C, and in the times, as shown in D of the same diagram, is very satisfactory.

The difference in the periods of the tide at which the waves occurred would tend to cause discrepancies.

The first series occurred on a rising tide of 4 feet, while at San Francisco it was upon a falling one of 2 feet. The second began near high water, and was chiefly upon a falling tide of 7 feet; while at San Francisco it was upon a rising tide of 4 feet.

The forms of some of the individual waves in the second series at San Francisco and San Diego accord remarkably, as those marked 1, 3, 4, 5, and 6, when reduced to the horizontal line. The comparison on the curve where the distortion remains is also very instructive. The waves marked 1, 4, 6, and 7 are not unlike in the first and second sets at San Diego.

The observations at San Diego confirm then, in general, the inferences derived from those at San Francisco.

The register at Astoria throws no new light upon the subject. The bar at the entrance of the Columbia River would explain why the oscillations were lost or greatly reduced at Astoria, even if they arrived off the entrance of the river. The disturbance is marked on the register, but in an irregular and confused manner. It was also, apparently, preceded by unusual oscillations of the water.

After allowing for the very free action of the float of the San Diego gauge, there appear to have been indications of disturbance previous to the great earthquake shocks and following them, occurring at intervals for several days after the 23d of December. The San Francisco gauge presents similar indications.

No special effect appears to have been produced upon the time or height of high or low water by the earthquake, which merely caused series of oscillations upon the great tidal wave.

I now proceed to draw from these results some conclusions as to the progress of the ocean wave accompanying the earthquake.

The latitudes and longitudes of the places referred to are as follows: —

	Latitude N.	Longitude.	W.
	° ′	° ′	h. m.
San Diego, . .	32 42	117 13	7 49
San Francisco, .	37 48	122 26	8 10
Simoda, . . .	34 40	221 2	14 44

The distance from San Diego to Simoda from these data is 4,917 nautical miles, and from San Francisco to Simoda, 4,527 nautical miles.

According to one account, the disturbance began at Simoda at 9 A. M., or $22^{d.}\ 23^{h.}\ 44^{m.}$ Greenwich mean time, and the first great wave, half an hour after. The first disturbance at San Francisco was at $23^{d.}\ 4^{h.}\ 12^{m.}$, or $12^{h.}\ 28^{m.}$ after that at Simoda, and the first great wave at $23^{d.}\ 4^{h.}\ 42^{m.}$, giving the same interval. The distance and time from this account give for the rate of motion of the wave 363 miles per hour, or 6 miles per minute. The second account would give for the time of transmission $12^{h.}\ 13^{m.}$, and for the rate of motion 370 miles per hour, or 6.2 miles per minute.

The San Diego observations give for the time of transmission of the wave from Simoda to San Diego $13^{h.}\ 50^{m.}$ by the first account, which, combined with the distance, gives 355 miles per hour, or sensibly the same result as derived from the beginning at San Francisco. The first great wave would give identically the same result.

From the results obtained, we may determine the mean depth of the Pacific Ocean in the path of the earthquake waves. We have found for the rate of motion from 6 to 6.2 miles per minute, and for the duration of an oscillation 35 minutes at San Francisco, and 31 at San Diego. This would give for the length of the wave on the San Francisco path 210 to 217 miles; and on the San Diego path, 186 to 192 miles. A wave of 210 miles in length would move with a velocity of 6 miles per minute in a depth of 2,230 fathoms (Airy, *Tides and Waves*, Encyc. Metrop., p. 291, Table II.); one of 217 miles, with a velocity of 6.6 miles per minute in a depth of 2,500 fathoms. The corresponding depth on the San Diego path is 2,100 fathoms.

The disturbance of the 25th of December presents at San Francisco three sets of waves of seven each, and at San Diego one set of seven, agreeing in their general features with those at San Francisco, and then a set of seventeen, in which, at first, intermediate waves seem to be wanting at San Francisco, or which have no analogous oscillations there. The crests of the first set occurred at a mean about $17^{m.}$ earlier at San Diego than at San Francisco; the heights on the average were nearly the same, being 0.39 ft. at San Diego and 0.44 ft. at San Francisco, and the time of oscillation at the two places the same, namely $41^{m.}$ The origin of the disturbance was probably nearer to San Diego than to San Francisco.

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4. Discussion of the Secular Variation in the Magnetic Declination on the Atlantic and Gulf Coast of the United States, from Observations in the 17th, 18th, and 19th Centuries. By Charles A. Schott, U. S. Coast Survey. [Abstract from a Report to the Superintendent of the Coast Survey, dated July 6th, 1855, and communicated by authority of the Treasury Department.]

This investigation was undertaken with a view of deducing the reduction to the same epoch of any of the Coast Survey observations for magnetic declination, and also with a view of predicting or calculating the declination for positions occupied prior to the present time, as well as to restore from present observations the declination at some earlier date.

The extensive use of the compass in the surveys of public lands renders a knowledge of the law of change in the direction of the needle during this and the latter half of the last century an object of great importance, the aid of which law is not unfrequently required in legal proceedings. Though an investigation of the observations taken during the last decennium would have furnished approximate results for the immediate purposes of the survey, yet it is apparent that no general law could be deduced in this way, and it became necessary at once to include all available material, from the earliest time to the present. The discussion is based upon 180 observations taken at stations distributed over the Atlantic and Gulf coast.

In reference to terrestrial magnetism in general, Professor Hansteen has lately published his investigations on the secular variation of the magnetic inclination, in the *Astronomische Nachrichten*, Nos. 947, 948, and 954. (See for a short abstract, *Comptes Rendus*, Tom. XL. No. 15.) The appearance of this paper, and the necessity of the reduction of our observations for declination published in the Superintendent's annual report for 1854, gave a new impulse to this and similar investigations.

Beyond the fact of the nearly stationary condition of the direction of the needle about the commencement of this century in the North-eastern States, and the observed increase of westerly declination in opposition to the former decrease of the same in the New England States, little was known in reference to the law of the secular change, either in time or in geographical relation. It is to Dr. Bowditch and Professor Loomis that we are mostly indebted on this subject; to the former for having called attention to the phenomenon at the time when the change from the direct to the retrograde motion took place, to the latter for a collection of numerous observations of magnetic declinations in the United States, and also for two charts of isogonic lines for the years 1838 and 1840. Professor Loomis states that all the observations indicate a retrograde motion of the needle, which commenced as early as 1819, and in some places perhaps as early as 1793. "The present (1840) annual change of the variation is about 2 minutes for the Southern States, 4 minutes for the Middle, and 6 for the New England States." (See Silliman's Journal of Science and Art, Vol. XXXIX., 1840.)

In the following discussion I have used nearly all the data I was able to collect. There can be no doubt that much additional information might be obtained from the surveys of public lands, as their results generally are derived from a number of observations at different places, and for this reason are more likely to be free from any local deviation, the effect of which is more to be guarded against than errors of observation. Results obtained by the ordinary surveyor's compass thus show at the stations Providence, Hatboro', and others, the best agreement. In order to obtain reliable results for the secular change, it is essential that the observations should be made at the same spot; but this is seldom the case, and to this circumstance differences, amounting in some instances to half a degree and more, must be attributed.

The observations at stations mentioned in the following pages have been discussed in three different ways, depending on the dates of their commencement and termination. Those prior to the middle of the eighteenth century require, as will be seen in the discussion, a function involving an additional term in the expression for the declination; to this class belong the stations Providence, R. I., Hatboro', Pa., and Philadelphia. Others reaching as far back as these are too discordant for use. Observations taken on shipboard are unreliable on account of local attraction, and hence have not been employed in the discussion. The second class includes observations made subsequent to the middle of the eighteenth century at the three stations before named, and at seven others, and reaching to the present time. The third class includes all stations having two or more observations of comparatively recent date; and these, as may be remarked, are less important for deducing the secular change than for the construction of the isogonic lines.

Throughout the discussion, westerly declination has been considered as positive, and easterly as negative. The formulæ used, being the same for all stations, require but once to be explained, and are given in full in the discussion of the first station of the first and second class. All observations have been scrutinized, and the references are affixed to the results. The separate heads into which the subject divides itself are as follows: —

a. Discussion of the secular change at stations with reliable observations dating prior to about the year 1740.

b. Discussion of the secular change at stations with reliable observations dating after that time.

c. Statement of results from comparatively recent observations.

d. Establishment of formulæ expressing the secular variation of the magnetic declination at any place within the limits of the discussions. Synopsis of results, and general remarks.

We commence with the discussion of the observations comprised under the head *a.*

a. Discussion of the Secular Variation of the Magnetic Declination, from the oldest reliable Observations, viz. those recorded at Providence, R. I., Hatboro', Pa., and Philadelphia.

The first-named set includes 30 observations made between 1717 and 1845; the second, commencing with the year 1680, presents 18 observations made at equal intervals, terminating with the year 1850; the third contains 10 observations recorded between the years 1701 and 1847.

Method and Formulæ for the Reduction.

The magnetic declination D at the time t may be expressed by the following series:

$$D = d_0 + y(t + t_0) + z(t - t_0)^2 + u(t - t_0)^3 + \ldots\ldots$$

where $y\ z\ u \ldots$ are unknown co-efficients and D becomes d_0 when t equals t_0.

Putting $d_0 = d_1 + x$, where x is a small correction to the assumed value d, and omitting the 4th and higher powers of the time, the above equations become

$$D = d_1 + x + y(t - t_0) + z(t - t_0)^2 + u(t - t_0)^3.$$

If we assume for t_0 the commencement of any year, and for d_1 the supposed corresponding declination (expressed in degrees and decimals), then each observed value for D at the time t furnishes an equation of the form

$$0 = d_1 - D + x + y(t - t_0) + z(t - t_0)^2 + u(t - t_0)^3,$$

and known as a conditional equation. By application of the method of least squares, we form the normal equations, and obtain the co-efficients x, y, z, u.

The above formula is capable of giving two maxima and two minima, whereas the omission of the third power would give but a minimum, and this, as we know from observation, took place about the commencement of this century. The omission of the term involving the third power constitutes the difference of the classifications *a* and *b*.

The year 1830 has been assumed throughout the discussions for the arbitrary value t_0, for a reason which will appear in the comparison of the results at different stations.

Let ϵ_0 be the probable error of a single observation,

n, the number of observations,

Δ, the difference of observed and computed values;

then
$$\epsilon_0 = 0.674\sqrt{\frac{\Sigma\Delta^2}{n-4}}.$$

Differentiating the equation

$$D = d_0 + y(t - t_0) + z(t - t_0)^2 + u(t - t_0)^3,$$

we obtain the condition for the maximum and minimum:

$$\frac{d\,D}{d\,t} = y + 2\,z\,(t - t_0) + 3\,u\,(t - t_0)^2 = 0.$$

Changing t into τ and τ' for the time of the minimum and maximum, we find,

$$\tau = t_0 - \frac{z}{3\,u} + \sqrt{\left(\frac{z}{3\,u}\right)^2 - \frac{y}{3\,u}}, \text{ and } \tau' = t_0 - \frac{z}{3\,u} - \sqrt{\left(\frac{z}{3\,u}\right)^2 - \frac{y}{3\,u}}.$$

The point of inflexion, or the time of maximum annual variation, will be found by putting the second differential co-efficient zero:

$$\frac{d^2D}{d\,t^2} = 2\,z + 6\,u\,(t - t_0) = 0.$$

Changing t into τ'' for the time of maximum annual variation, we have

$$\tau'' = t_0 - \frac{z}{3\,u}.$$

The maximum declination δ becomes known by substituting τ' for t in the formula for D.

The first differential co-efficient gives the formula for the annual variation v,

$$v = y + 2\,z\,(t - t_0) + 3\,u\,(t - t_0)^2.$$

Substituting τ'' for t, we find the maximum annual change V.

We have next to find the probable errors of the quantities x, y, z, u, τ, τ', τ'', &c., by forming the weight-equations, which will give the necessarily positive quantities Q_1, Q_2, Q_3, Q_4.

ϵ expresses generally a probable error, and its index indicates to which quantity this probable error refers. We have

$$\epsilon_x = \epsilon_0\,\sqrt{Q_1},$$
$$\epsilon_y = \epsilon_0\,\sqrt{Q_2},$$
$$\epsilon_z = \epsilon_0\,\sqrt{Q_3},$$
$$\epsilon_u = \epsilon_0\,\sqrt{Q_4}.$$

To find the probable error of τ, τ', τ'', we differentiate the expression for τ, τ', τ'' in regard to the variables x, y, z, u:

$$d\,\tau = -\frac{1}{6\,u\,A}\,d\,y - \left(\frac{1}{3\,u} - \frac{2\,z}{18\,u^2A}\right)d\,z + \left(\frac{z}{3\,u^2} + \frac{\frac{2\,z^2}{9\,u^3} - \frac{y}{3\,u^2}}{2\,A}\right)d\,u;$$

$$d\,\tau' = +\frac{1}{6\,u\,A}\,d\,y - \left(\frac{1}{3\,u} + \frac{2\,z}{18\,u^2A}\right)d\,z + \left(\frac{z}{3\,u^2} - \frac{\frac{2\,z^2}{9\,u^3} - \frac{y}{3\,u^2}}{2\,A}\right)d\,u;$$

$$d\tau'' = -\frac{1}{3u}dz + \frac{z}{3u^2}du;$$

in which expressions $A = \sqrt{\left(\frac{z}{3u}\right)^2 - \frac{y}{3u}}$.

For the above equations we can substitute

$$d\tau = l_1\,dy + l_2\,dz + l_3\,du;$$
$$d\tau' = l_1'\,dy + l_2'\,dz + l_3'\,du;$$
$$d\tau'' = \qquad l_2''\,dz + l_3''\,du;$$

hence,

$$\epsilon_\tau = \sqrt{l_1\,l_1\,\epsilon_y\,\epsilon_y + l_2\,l_2\,\epsilon_z\,\epsilon_z + l_3\,l_3\,\epsilon_u\,\epsilon_u};$$
$$\epsilon_{\tau'} = \sqrt{l_1'\,l_1'\,\epsilon_y\,\epsilon_y + l_2'\,l_2'\,\epsilon_z\,\epsilon_z + l_3'\,l_3'\,\epsilon_u\,\epsilon_u};$$
$$\epsilon_{\tau''} = \sqrt{l_2''\,l_2''\,\epsilon_z\,\epsilon_z + l_3''\,l_3''\,\epsilon_u\,\epsilon_u};$$

The differential equation

$$dv = dy + 2\,(t - t_0)\,dz + 3\,(t - t_0)^2\,du$$

gives the value for

$$\epsilon_v = \sqrt{\epsilon_y^2 + 4\,(t - t_0)^2\,\epsilon_z^2 + 9\,(t - t_0)^4\,\epsilon_u^2}.$$

Finally, we have

$$\epsilon_D = \sqrt{\epsilon_x^2 + (t - t_0)^2\,\epsilon_y^2 + (t - t_0)^4\,\epsilon_z^2 + (t - t_0)^6\,\epsilon_u^2}.$$

By means of these formulæ the observations have been discussed.

Discussion of the Secular Change at Providence, R. I.

This is a very important station, both in regard to the number and the agreement of the observations. In Vol. XLIV. of Silliman's Journal of Science and Art, 1843, a series of observations have been published, under the title, "The Variation of the Magnetic Needle at Providence, R. I., from A. D. 1717 to 1843, by M. B. Lockwood, C. E., from actual Observations on Record, and recorded Bearings of a Number of Objects."

Providence is in lat. 41° 49′ and long. 71° 24′ W.

These observations, when treated by the method just explained, are best represented by the formula

$$D = +7^\circ.437 + 0.08543\,(t - 1830) + 0.0015055\,(t - 1830)^2 + 0.000005100\,(t - 1830)^3.$$

The following table shows the differences between the observed and computed declinations: —

t	D observed.	D computed.	Δ	Δ^2
1717	+ 9.60	+ 9.64	+ 0.04	0.0016
1720	+ 9.47	+ 9.46	— 0.01	0.0001
1730	+ 8.92	+ 8.85	— 0.07	0.0049
1740	+ 8.28	+ 8.22	— 0.05	0.0025
1750	+ 7.67	+ 7.62	+ 0.05	0.0025
1760	+ 6.99	+ 7.08	+ 0.09	0.0081
1770	+ 6.49	+ 6.63	+ 0.14	0.0196
1780	+ 6.27	+ 6.29	+ 0.02	0.0004
1790	+ 6.18	+ 6.10	— 0.08	0.0064
1800	+ 6.25	+ 6.09	— 0.16	0.0256
1810	+ 6.40	+ 6.29	— 0.11	0.0121
1820	+ 6.66	+ 6.73	+ 0.07	0.0049
1830	+ 7.19	+ 7.44	+ 0.25	0.0625
1840	+ 8.42	+ 8.45	+ 0.03	0.0009
1842	+ 8.65	+ 8 69	+ 0.04	0.0016
1844.8	+ 9.25	+ 9.05	— 0.20	0.0400

Treating the observations at the other stations in a similar manner, we obtain the following results: —

Synopsis of Results at the Stations Providence, Hatboro', and Philadelphia.

Providence, . . . $D = +7^\circ.439 + 0.08543\,(t - 1830) + 0.001505\,(t - 1830)^2 + 0.00000510\,(t - 1830)^3$.

Hatboro', ... $D = +2^\circ.683 + 0.07211\,(t - 1830) + 0.001749\,(t - 1830)^2 + 0.00000675\,(t - 1830)^3$.

Philadelphia, . . . $D = +2^\circ.573 + 0.06582\,(t - 1830) + 0.001838\,(t - 1830)^2 + 0.00000742\,(t - 1830)^3$.

For t, any year might be substituted between 1670 or 1680 and the present time. The agreement of the co-efficients is satisfactory, at the same time exhibiting their dependence on the geographical position of the stations.

		V
Providence, .	$v = +\overset{\circ}{0}.085 + 0.00301\,(t - 1830) + 0.0000153\,(t - 1830)^2$.	3'.8
Hatboro', . .	$v = +0.072 + 0.00350\,(t - 1830) + 0.0000203\,(t - 1830)^2$.	4.8
Philadelphia,	$v = +0.066 + 0.00368\,(t - 1830) + 0.0000223\,(t - 1830)^2$.	5.2

	τ	τ'	τ''	δ	d	Range.
Providence, . . .	1795.6	1667.7	1731.6	$+11^\circ.5$	$+6^\circ.1$	$5^\circ.4$
Hatboro', ...	1806.1	1681.3	1743.6	+ 8.5	+ 1.9	6.6
Philadelphia, . . .	1809.5	1688.3	1747.4	+ 9.0	+ 1.9	7.1

	ϵ_0	ϵ_x	ϵ_y	ϵ_z	ϵ_u
Providence, .	± 5'	± 0.035	± 0.00199	± 0.000057	± 0.00000044
Hatboro', . . .	± 11	± 0.077	± 0.00414	± 0.000040	± 0.00000070
Philadelphia, . .	± 24	± 0.179	± 0.01100	± 0.000250	± 0.00000220

	e_τ	$e_{\tau'}$	$e_{\tau''}$	e_V
Providence, ...	± 6.1 years.	± 9.6 years.	± 9.3 years.	± 1'.0
Hatboro', ...	± 19.3 "	± 5.0 "	± 12.9 "	± 1.0
Philadelphia, . . .	± 16.7 "	± 26.5 "	± 26.9 "	± 4.0

According to the results deduced from the observations made at these three stations, the minimum declination took place in 1804 ± 9 years. At this time the needle had approached nearest to the true north, the western declination being greater before and after this time. The maximum variation appears to have occurred in 1679 ± 10 years. Hence the duration of half an oscillation, if we are allowed to draw the inference, would be 125 years ± 15; but this must at present be considered as speculative. The uniformity in the epoch of the minimum for a great geographical extent in a north and south direction, as we shall see farther on, would lead to the inference of a constant duration of half an oscillation in the geographical direction indicated. At Paris and London the maximum of eastern declination (equivalent to a minimum of western) took place about 1680, and the maximum of western declination about 1815, with a range of not less than 34°, while the average range at the above three stations is but 6°. The maximum of westerly declination in the northwestern part of Europe, therefore, took place nearly simultaneously with the minimum westerly declination on the eastern coast of the United States. I shall return to this subject after the discussion of the observations comprised under the head *b*. The maximum annual variation *V* took place in 1741 ± 10 years, and if after an equal interval of time $\tau - \tau''$, or 63 years, the greatest annual change should again take place, we must expect it about the year 1867 ± 15 years. Observations made during the present year indicate an increasing change, so far supporting this conjecture. The average value of *V* for the three stations is 4'.6, showing at the same time an increase with an approach to the line of no variation.

b. Discussion of the Secular Variation of Magnetic Declination from reliable Observations dating subsequent to the Year 1740, *with others made prior to that Time.*

From the preceding discussion, it is obvious that all the observations after the time τ'' can be represented by an equation of the second degree, which will give the epoch of the minimum, its corresponding and all other declinations between that time (the former point of inflection of the curve) and the present. This formula will apply up to the time of a second point of inflection, yet to be observed.

Although this class includes stations with observations reaching considerably beyond the middle of the eighteenth century, yet for want of general conformity such have been omitted in the discussion. For the purpose of a ready comparison of the *co-efficients* of the terms involving the interval of time for the several stations, and for the purpose of ascertaining their change with reference to geographical position, a rediscussion of the preceding three stations becomes necessary, in which the observations after 1740 alone are used.

The stations have been arranged in the order of their geographical position, commencing with the observations in the New England States.

No.	Station.	Observations included between	Number of Observations.
1	Boston, Mass.	1700 and 1847	8
2	Cambridge, Mass.	1708 " 1855	20
3	Providence, R. I.	1740 " 1845	25
4	New Haven, Conn.	1761 " 1849	13
5	New York, N. Y.	1609 " 1846	12
7	Hatboro', Pa.	1750 " 1850	11
6	Philadelphia, Pa.	1750 " 1846	8
8	Charleston, S. C.	1775 " 1849	5
9	Mobile, Ala.	1809 " 1850	5
10	Havana, Cuba,	1726 " 1850	3

Treating these observations by the preceding formulæ, we find the following results: —

Synopsis of Results of the Discussion for Secular Variation at the 10 *preceding Stations.*

No.	Station.	Lat.	Long.	Declination.
		° ′	° ′	°
1	Boston,	42 20	71 2	$D=+8.356+0.0647\,(t-1830)+0.000624\,(t-1830)^2$
2	Cambr.	42 23	71 7	$D=+8.553+0.0702\,(t-1830)+0.000720(t-1830)^2$
3	Provid.	41 49	71 24	$D=+7.575+0.0764\,(t-1830)+0.000959\,(t-1830)^2$
4	N. Haven,	41 18	72 55	$D=+5.395+0.0500(t-1830)+0.000857\,(t-1830)^2$
5	N. York,	40 43	74 0	$D=+5.071+0.0642\,(t-1830)+0.000944\,(t-1830)^2$
6	Hatboro',	40 7	75 8	$D=+2.861+0.0683(t-1830)+0.001169\,(t-1830)^2$
7	Philadel.	39 58	75 10	$D=+2.599+0.0684(t-1830)+0.001340\,(t-1830)^2$
8	Charlest.	32 45	79 51	$D=-3.330+0.0485\,(t-1830)+0.000722\,(t-1830)^2$
9	Mobile,	30 14	88 0	$D=-7.238+0.0072\,(t-1830)+0.000123\,(t-1830)^2$
10	Havana,	23 9	82 22	$D=-6.076+0.0098(t-1830)+0.000255\,(t-1830)^2$

No.	Station.	ϵ_0	τ	ϵ_τ	d	Annual Variation.
1	Boston,	± 13'	1778.2	± 11.3 yrs.	+ 6°.68	$v = + 0°.065 + 0.00125\ (t - 1830)$
2	Cambr.	± 5	1781.2	± 1.8 "	+ 6.83	$v = + 0.070 + 0.00144\ (t - 1830)$
3	Provid.	± 8	1790.1	± 2.2 "	+ 6.05	$v = + 0.076 + 0.00192\ (t - 1830)$
4	N. Haven,	± 12	1800.8	± 4.1 "	+ 4.67	$v = + 0.050 + 0.00171\ (t - 1830)$
5	N. York,	± 16	1796.0	± 6.1 "	+ 3.98	$v = + 0.064 + 0.00189\ (t - 1830)$
6	Hatboro',	± 8	1799.5	± 1.2 "	+ 1.87	$v = + 0.068 + 0.00224\ (t - 1830)$
7	Philadel.	± 20	1804.5	± 3.1 "	+ 1.69	$v = + 0.068 + 0.00268\ (t - 1830)$
8	Charlest.	± 10	1796.4	......	— 4.16	$v = + 0.048 + 0.00144\ (t - 1830)$
9	Mobile,	(± 6)	1800.6	......	— 7.35	$v = + 0.007 + 0.00024\ (t - 1830)$
10	Havana,	(± 5)	1810.8	......	— 6.17	$v = + 0.010 + 0.00051\ (t - 1830)$

No.	Station.	v in 1850.	ϵ_v	ϵ_x	ϵ_y	ϵ_z
1	Boston,	+ 5'.4	± 0'.5	± 8'	± 0.0061	± 0.00016
2	Cambridge,	+ 5.9	± 0.1	± 2	± 0.0017	± 0.00002
3	Providence,	+ 6.9	± 0.2	± 3	± 0.0028	± 0.00004
4	New Haven,	+ 5.0	± 0.4	± 4	± 0.0047	± 0.00009
5	New York,	+ 6.2	± 0.6	± 7	± 0.0080	± 0.00012
6	Hatboro',	+ 6.8	± 0.1	± 3	± 0.0008	± 0.00005
7	Philadelphia,	+ 6.8	± 0.5	± 8	± 0.0082	± 0.00011
8	Charleston,	+ 4.6	.....	....	....	....
9	Mobile,	+ 0.7	.....	....	....	....
10	Havana,	+ 1.2	.....	....	....	....

The order in which the stations have been arranged serves to show the dependence of the co-efficients $x\ y\ z$ upon the geographical position of the stations; but the last two co-efficients will be investigated further on, as on these alone depends the secular change and the time of the minimum.

The epoch of the minimum (τ) appears to be subject to local irregularities, as disclosed by the probable errors (ϵ_τ), and a general law or dependence on the same cause is strongly marked as affecting every station on the Atlantic seaboard, and is even traceable from the southern shore of Hudson's Bay to Jamaica. The mean τ, without regard to the probable error, is 1796, and when we form the differences, τ — mean, the figures at once show that in the Eastern States the minimum of magnetic declination took place about a decennium earlier, and about the same number of years later, in the Eastern Gulf States, than in the Middle States. In the last section this geographical relation of the co-efficients y and z, and of the epoch τ, will be more fully investigated. It is no more suprising to find local deviations in the epoch of the minimum, than in the declination itself.

The general flatness of the curves, as we approach the Gulf of Mexico, is remarkable, and induced me to collect a few observations for declination at Jamaica, W. I., lat 17° 58′, long. 76° 46′. Permanent declination at Kingston, Jamaica, from 1660 to 1800, 6°.30′ E. (J. Robertson, Phil. Trans. Royal Society, 1806). Declination from a plan of Kingston, by J. Leard, in 1791 and 1792, 6° 45′ E., and by the same authority in 1789 and 1793, 6° 50′ E. On Minories' map, published in London, 1854, the declination is given 4° 40′ E., which is probably for 1833. Colonel Sabine, in the Phil. Trans. Royal Society, Part II. 1849, Contrib. IX., gives the declination —3°.8 and —4°.2, the mean of which is 4° E. (for 1840?) The latter two determinations show that there is an end to the permanency in the direction of the needle since the commencement of this century. Sir John Herschel says: "The whole mass of West Indian property has been saved from the bottomless pit of endless litigation by the invariability of the magnetic declination in Jamaica and the surrounding Archipelago during the whole of the last century; all surveys of property there have been conducted solely by the compass." Examining, on the other hand, the declinations at Fort Albany, at the southern extremity of Hudson's Bay, lat. 52° 22′, long. 82° 38′, we find declination in 1668, according to Halley, 19° 15′ W. (see Hansteen's *Erdmagnetismus*, Vol. I.); declination in August, 1730, according to Captain Middleton, 23° 0′ W.; and by the same authority, declination in Sept. 1774, 17° 0′ W. Hansteen's map for 1787 gives 14° W., and Barlow's map for 1833 (Phil. Trans.) 3° W. We may here particularly notice the maximum, which must have taken place between 1668 and 1730. Hence we see that the curves for these extreme stations agree well in their general character with the previous investigation, which is thereby considerably expanded in the direction of the meridian, and it becomes a matter of interest to examine the same in the direction of the parallel, which, however, does not come within the compass of this paper.

c. *Statement of Results from comparative recent Observations and Discussion of some Anomalous Stations.*

The discussion at Burlington, Vt., lat. 44° 28′, long. 73° 14′, has shown that the curvature appears to be greater than usual, and that the minimum is displaced to 1808 ± 4. The observations are represented by

$$D = +8^\circ.363 + 0.1207\,(t - 1830) + 0.002755\,(t - 1830)^2.$$

At Chesterfield, N. H., lat. 42° 53′, long. 72° 20′, the same late minimum, 1814.1, has been deduced by the formula,

$$D = +7^\circ.040 + 0.1053\,(t - 1830) + 0.003289\,(t - 1830)^2.$$

At Salem, Mass., lat. 42° 31′, long. 70° 54′, the curvature is again very considerable, as seen by the co-efficient of the expression,

$$D = +7^\circ.420 + 0.1235\,(t - 1830) + 0.002137\,(t - 1830)^2.$$

The disturbed region around Cape Ann, extending as far as Salem, is also manifested in the secular variation.

At Nantucket, Mass., lat. 41° 17′, long. 70° 6′, the observations are represented by

$$D = +8^\circ.232 + 0.0612\,(t - 1830) + 0.000534\,(t - 1830)^2,$$

but the minimum falls very early; the formula gives

$$\tau = 1772.8.$$

For Albany, N. Y., lat. 42° 39′, long. 73° 44′, we find

$$D = +6^\circ.356 + 0.0682\,(t - 1830) + 0.00128\,(t - 1830)^2,$$

and

$$\tau = 1803.4.$$

At Washington, D. C., and Pensacola, Fa., the observations are as yet too few in number to be submitted to a discussion, but this may be done as soon as a new determination can be had. The same may be said of Milledgeville, Ga., Savannah, Ga., and New Orleans, La.

New observations at the stations Burlington, Nantucket, Albany, and perhaps Chesterfield, will render them available for the general discussion. The early minimum at Nantucket is probably as anomalous as the late minimum at Chesterfield. The preceding discussion, with the exception of the result at Nantucket, tends again to a later minimum for stations to the northward of New York and Boston. The best value, at present deducible, for the epoch of the minimum, including the whole geographical extent, may be assumed to be the general mean of τ, which is 1797.2 ± 8.0 years.*

* From observations down to September, 1855, the average τ becomes 1797.6 ± 1.8 years.

d. Establishment of Formulæ expressing the Secular Variation in the Magnetic Declination at any Place within the Geographical Limits of Stations named in the Discussions.

We have seen that the co-efficients in the formula for the magnetic declination depend upon the geographical position of the station, and it now remains to express this dependence analytically.

The declination has been expressed by the formula

$$D = C + C' (t - 1830) + C'' (t - 1830)^2,$$

where C is a constant (the former x) and $C' + C''$ stands for the former co-efficients y and z.

Now C' may be expressed by a formula involving a constant term and terms of differences of latitude and longitude, namely,

$$C' = c' + x + y (l - L) + z (m - M) \cos l + u (l - L)^2 + v (m - M)^2 \cos^2 l,$$

where c' is an average value of all the C', L the mean latitude, M the mean longitude, and x, y, z, u, v, co-efficients to be determined.

The following table has been formed from the preceding discussion:

Station.	l	m	$10000\ C'$	$1000000\ C''$
	°	°		
Boston,	42.3	71.0	647	624
Cambridge,	42.4	71.1	702	720
Providence,	41.8	71.4	764	959
New Haven,	41.3	72.9	500	857
New York,	40.7	74.0	642	944
Hatboro',	40.1	75.1	683	1169
Philadelphia,	40.0	75.2	684	1340
Charleston,	32.8	79.8	485	722
Mobile,	30.2	88.0	72	123
Havana,	23.1	82.4	98	255
Mean,	$L = 37.5$	$M = 76.1$	$c' = 528$	$c'' = 771$

Putting $l - L = \lambda$, $(m - M) \cos l = \mu$, and $c' - C' = \Delta'$, we obtain the conditional equation

$$0 = \Delta' + x + y\lambda + z\mu + u\lambda^2 + v\mu^2.$$

Substituting the above values, and forming the conditional and normal equations, we find the values for x, y, z, u, and v, as follows:

$$x = +79;\ y = +24;\ z = +2.5;\ u = -0.28;\ \text{and}\ v = -3.42.$$

Hence the expression for C' becomes

$$C' = +0.0607 + 0.00240\lambda + 0.00025\mu - 0.000028\lambda^2 - 0.000342\mu^2.$$

The original equations are represented as follows:—

Station.	C'	C' comp'd.	Δ
Boston,	0.0647	0.0658	+ 0.0011
Cambridge,	0.0702	0.0663	— 0.0039
Providence,	0.0764	0.0655	— 0.0109
New Haven,	0.0500	0.0668	+ 0.0168
New York,	0.0642	0.0669	+ 0.0027
Hatboro',	0.0683	0.0665	— 0.0018
Philadelphia,	0.0684	0.0668	— 0.0016
Charleston,	0.0485	0.0463	— 0.0022
Mobile,	0.0072	0.0080	+ 0.0008
Havana,	0.0098	0.0114	+ 0.0016

This is a satisfactory agreement, as appears from a comparison of the average probable error ϵ_y (of a former table), and the probable error of C', as deduced from the above Δ.

We have $\epsilon_y = \pm\, 0.0046$, and $\epsilon_{c'} = 0.674 \sqrt{\frac{\Delta^2}{n-5}}$;
$\epsilon_{c'} = \pm\, 0.0063$;

hence the above formula represents the co-efficient C' nearly as close as it was itself deduced at the separate stations.

Similarly the second co-efficient C'' may be expressed by

$$C'' = c'' + x + y(l - L) + z(m - M)\cos l + u(l - L)^2 + v(m - M)^2\cos^2 l$$

and

$$0 = \Delta'' + x + y\lambda + z\mu + u\lambda^2 + v\mu^2.$$

We find

$$C'' = +\,0.000850 + 0.000196\,\lambda + 0.000251\,\mu + 0.000008\,\lambda^2 - 0.000023\,\mu^2.$$

This formula does not represent the values of C'' as closely as we might have expected, yet differences of 0.000200 might have been anticipated from an inspection of C'' for Hatboro' and Philadelphia. The greatest difference is for Providence, namely, —0.000274. When the station for which C'' is to be found is situated within the range of the position of the above places, the formula may be applied; yet it will be found preferable to make use of a more simple relation of the co-efficients C' and C''. Referring to the table showing C' and C'', their increasing ratio is apparent, and putting $C'' = n\,C'$, we have for

Boston, ... n =	0.010	Hatboro', ... n =	0.017
Cambridge, . . . "	0.010	Philadelphia, . . "	0.018
Providence, . . . "	0.013	Charleston, ... "	0.015
New Haven, . . "	0.017	Mobile, ... "	0.017
New York, . . . "	0.015	Havana, ... "	0.026

which relation is sufficiently regular to allow the interpolation of any value derived within its range.

C' and C'' being thus known, an observed value of the declination at a given place will determine the constant C, and will enable us to deduce the declination for any time t. For this place the epoch of the minimum becomes known by the expression $1830 - \frac{C'}{2\,C''}$, and the annual variation by $v = C' + 2\,C''\,(t - 1830)$. For want of observations, the values deduced for C' and C'' must at present be considered as approximations.*

Before concluding this paper, it was thought proper to refer to a few circumstances closely related to the preceding discussion, and tending to modify former conclusions.

1. In the Phil. Trans. Royal Society, Vol. XI. (abrid.) from 1755 to 1763, we find the paper on the variations which Professor Hansteen has made use of in the construction of some of his charts of isogonic lines in his *Erdmagnetismus*. The following is an abstract of a small part of this paper, "On the Variation of the Magnetic Needle, with a set of tables exhibiting the result of upwards of 50,000 observations, in six periodic reviews, from the year 1700 to the year 1756, and adapted to every fifth degree of latitude and longitude. By W. Mountaine and J. Dodson, F. F. R. S."

Lat.	Long.	Declination in			
		1700.	1730.	1744.	1756.
25°	80°	4¾° E.		3½° E.	3° E.
30	80	2½ E.		¾ E.	0
35	75	2¼ W.		6½ W.	7 W.
40	70	7 W.	9° W.	11½ W.	12¾ W.

Now we know from the preceding discussion, that the western declination (see the two last lines in the above table) had been *decreasing* since 1700, reached a maximum annual decrease in 1744, and continued decreasing down to about 1797, while the above table gives an *increase* during this interval of time, and is therefore entirely at variance with the observations taken on land. From this cause some

* From observations down to September, 1855, the following expression of C' is deduced: $C' = + 0.0556 - 0.00104\,\lambda - 0.00444\,\mu - 0.000165\,\lambda^2 - 0.000008\,\mu^2$.

of the geographical representations in the *Erdmagnetismus* based thereon require considerable corrections.

2. The rate of secular change used for the lines of equal magnetic declination in the Atlantic by Colonel Sabine, Phil. Trans. Royal Society, 1849, was derived from comparison with the map of declinations for 1787 in Professor Hansteen's work referred to above. This assumes a uniform progressive rate of the secular change, which, though applicable for other places, St. Helena for instance (see Colonel Sabine's paper, read May 18th, 1854), is, as we have seen, entirely inadmissible on our Atlantic coast, and may even give no rate at all for the time of the maximum rate. In consequence of this, the rates deduced in Table No. X. Trans. of 1849 are very much in error, and affect more or less the resulting isogonic lines depending on it. For lat. 40° long. 75° the table gives an annual variation for 1840 = 0'.0, when it should be +5'.7.

3. In Professor Hansteen's paper on the changes in the magnetic inclination in the north temperate zone, *Astronomische Nachrichten*, Nos. 947, 948, and 954, the declination is stated to have a retrograde motion, as inferred from Professor Loomis's table in Silliman's Journal, Vol. XXXIX., and consequently an easterly motion is assigned to the pole B (pages 187 and 192, No. 948). Again, on page 282, No. 954, it is said that in North America the *western* declination *decreases*, and the easterly increases; which, however, is not the case, as has been seen; for since the minimum, about the beginning of the present century, the reverse has taken place, the westerly declination having ever since *increased* (or the easterly diminished). Professor Hansteen's pole B, therefore, appears to have reached its most easterly position about 1797, and has ever since been moving to the *westward*.

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5. The Frozen Wells of Owego. By Professor John Brocklesby, of Trinity College, Hartford, Conn.

About sixteen years ago a letter was addressed to Professor Silliman, by Mr. D. O. Macomber, of Owego, Tioga Co., N. Y., describing certain unexplained phenomena pertaining to a well situated in the vicinity of Owego.

The communication of this gentleman (which is recorded in Vol. XXXVI. of the Journal of Science) is as follows:—

"The well is excavated on a table-land elevated about thirty feet above the bed of the Susquehanna River, and distant from it three fourths of a mile. The depth of the well, from the surface to the bottom, is said to be 77 feet; but for four or five months of the year the surface of the water is frozen so solid as to be entirely useless to the inhabitants. On the 23d of the present month (February), in company with a friend, I measured the depth, and found it to be 61 feet from the surface of the earth to the ice which covers the water in the well, and this ice we found it impossible to break with a heavy weight attached to a rope. The sides of the well are nearly covered with masses of ice, which, increasing in the descent, leave a space but one foot in diameter at the bottom.

"A thermometer let down to the bottom sunk 38° in 15 minutes, being 68° in the sun and 30° at the bottom of the well. The well has been dug 21 years, and I am informed by a very credible person, who assisted in the excavation, that a man could not endure to work in it more than two hours at a time, even with extra clothing, although in the month of June, and the weather excessively hot. The ice remains until very late in the season, and is often drawn up in the months of June and July. Samuel Mathews drew from the well a large piece of ice on the 25th of July, 1837, and it is common to find it there on the 4th of July.

"The well is situated in the highway, about one mile northwest of the village of Owego. There is no other well on that table of land, nor within 60 or 80 rods, and none that presents the same phenomenon. In the excavation no rock or slate was thrown up, and the water is never affected by freshets, and is what is usually denominated 'hard' or limestone water. A lighted candle being let down, the flame became agitated and thrown in one direction at the depth of 30 feet, but was quite still and was soon extinguished at the bottom. Feathers, down, or any light substance, when thrown in, sink with a rapid and accelerated motion. The above facts may be relied upon as entirely correct."

Such is the statement of Mr. Macomber. During last year I received from the Rev. Wm. H. Corning, who is settled at Owego, two letters in respect to this phenomenon, which show that it is not confined to one locality. In his first communication, dated June, 1854, Mr. Corning thus writes: "Two wells in Owego, some 60 feet deep, *freeze*, and within a week large lumps of ice have been drawn up from one of them. They are situated a short distance from the Susquehanna River, and are below its bed."

In my reply to this letter Macomber's well was mentioned, and certain observations and experiments suggested, which it was thought might tend towards the solution of the problem.

In the second letter of Mr. Corning, he writes as follows: —

"The deep well spoken of by Macomber is situated at some distance *west* of the village proper, about a *quarter of a mile* from Owego Creek, and is on rather high ground; but no hills or mountains of any height are in its vicinity. The water of this well in the month of September is said to be quite *warm;* so much so as to be unpalatable.

"The *other well* (from which the ice was drawn) is in the centre of the village, not more than *seventy yards* from the Susquehanna, and is about a *quarter of a mile* from any hill. The distance from the surface of the ground to the surface of the water in the well is, at this date (Aug. 19th), 22½ feet. The water is about 18 inches deep, and its temperature was, at noon of this day, 47° Fahr.

"By letting down a lighted candle, a slightly perceptible current of air is detected, and the flame blows nearly *west* (the river is *south*). For the space of 10 or 12 feet down from the mouth of the well, there is not the least current; but for the rest of the distance, to within a foot of the water, the current mentioned is about the same in force; yet hardly strong enough to settle the question as to lateral fissures.

"The sides of the well are coated with ice from early winter, and the water apparently freezes from the bottom and sides. Sometimes the surface is frozen so hard, that it becomes necessary to drop a heavy weight to break the ice, in order that water may be drawn; this, however, is only occasionally done. The former owner was obliged to remove a chain-pump, and resort to the old windlass, on account of the ice. The ice remains on the sides of the well until the middle of May, when it gradually melts away, and by about the middle of June it is almost entirely gone. The water of the well up to the middle of May is very cold, resembling ice-water.

"The formation of this region is what geologists call *alluvial*, I believe. For the depth of ten feet or more below the surface of the earth there are large quantities of pebble-stones, varying in size from that of a pea to those which measure 4 or 5 inches in diameter. These are mixed with gravel and a sandy loam, and farther down are veins or layers of gravel, free, for the most part, from these stones. The hill or mountain, from which the *village* well is about a quarter of a mile distant, is variously estimated by the inhabitants to be from 150 to 200 feet in height. Upon it are found shells and marl."

During the early part of this summer I received information similar to the above from the Rev. James Rankine, a gentleman who resides at Owego. Mr. Rankine remarks, in addition, that "when the ice begins to form in the cold weather, it can be seen forming under the surface of the water in shape like a basin; and that during last winter a cover was put upon the well, when all its usual phenomena disappeared."

I am not aware that any satisfactory explanation has yet been given of this singular phenomenon. Without claiming to solve it, the learned editor of the Journal of Science remarks, in respect to the well of Macomber, that "possibly the escape of compressed gas deep within the earth in the vicinity of the well, and in proximity to its waters, may account for the extraordinary low temperature that there prevails"; and this view he considers as perhaps countenanced by the slight current of air that exists in the well.

It is worthy of notice, that in *both* the wells the phenomena of the currents is the same; namely, that the flame of a candle is swayed in *one direction* before it reaches the bottom, where it is *still*, — facts that point towards a common cause.

A well somewhat similar to the Owego wells is found at Monte Video, a villa situated about 9½ miles from the city of Hartford, Ct.

This well is sunk through the loose trap rock and gravel to the depth of about 25 feet, and during the summer months the water that is drawn from it is as cool as iced water. So low is the temperature, that the teeth fairly ache with cold as the water is drank. In close proximity to this well is a lofty hill of trap, whose shelving sides are entirely composed of the *débris* of this rock.

In the celebrated cave of Orenburg, in Russia, we have another instance of a locality possessing in summer an extraordinary low temperature. This cavern, which is at the base of a hill of gypsum, and opens on the street of Orenburg, is partly filled with ice in the summer, the roof, which is broken by fissures, being hung with solid undripping icicles; and the hotter the weather, the more severe is the cold; while in the winter all the ice disappears.

In explanation of these phenomena, it is asserted that in summer, as in the case of mining shafts and galleries, a warm current of air *descends* through the channels and passages of the hill, evaporating the water that it meets with, and cooling so rapidly, that it is below the freezing point when it issues into the cave. In the winter the current would move in the opposite direction, the hill being *warmer* than the external atmosphere.

This explanation will not serve for the wells of Owego, though it possibly may for that at Monte Video; for the *former*, unlike the cave of Orenburg, are *coated with ice and frozen in winter*, and in *midsummer* their waters are as *warm* as those of other wells. Nevertheless, it appears from the statement of Mr. Rankine, that *evaporation* aids in some way in producing the extraordinary degree of cold that prevails in these wells; for when one of them was covered, and the evaporation checked, no ice was formed within it.*

The preceding facts have been presented, not for the purpose of now offering any solution of the problem they involve, but with the hope that their publicity may stimulate observation and inquiry, and elicit other facts which will lead us to a better understanding of this class of physical phenomena.

V. METEOROLOGY.

1. On the Storm which was experienced throughout the United States about the 20th of December, 1836. By Elias Loomis, Professor of Mathematics and Natural Philosophy in New York University.

Several years ago I undertook to investigate the phenomena of a violent storm which swept over the United States about the 20th of December, 1836; and the result of my investigations was published in

* The same phenomenon, according to report, was true of the Macomber well, which has now fallen in.

the seventh volume of the Transactions of the American Philosophical Society. This storm extended from the Gulf of Mexico to an unknown distance on the north. The fluctuation of the barometer increased with the latitude, at least as far as Quebec, the most northern point from which observations could be obtained. The area embraced in the observations included, therefore, only the southern half of the storm, and the phenomena of the northern half could only be supplied by conjecture. I therefore regarded the results of this investigation as unsatisfactory; but finding it impossible to obtain observations from more northern stations, I published the information which I had obtained; and sought for some more hopeful subject of investigation in the case of a storm which should be wholly embraced within the area of our observations. Such was the storm of February 4, 1842, with regard to which I was fortunate in obtaining abundant materials for investigation. In conducting this investigation, I adopted some peculiar methods, which, so far as I have been informed, had never been practised before. The phenomena of the storm, at intervals of twelve hours, were delineated on a series of maps of the United States, in such a manner that every important feature was made to appeal directly to the eye. First, those portions of the map corresponding to places where the sky was unclouded were colored blue; those portions where the sky was overcast, but without rain or snow, were colored brown; those portions of the country where rain was falling were colored yellow; and those portions where snow was falling were indicated by a green color. The direction of the wind was represented by arrows, and its force indicated by their length.

The observations of the barometer and thermometer were represented in the following manner. Having determined, as well as I was able, the mean height of the barometer at each station, I compared each observation with the mean. I then drew on the map a line passing through all those places where the barometer, at a given hour, stood at its mean height. This line was called the line of mean pressure. I then drew a line through all those places where the barometer, at the same hour, stood two tenths of an inch *above* the mean; another line through all those places where the barometer stood four tenths of an inch above the mean, &c. Similar lines were drawn through all those places where the barometer stood two tenths of an inch *below* the mean; four tenths of an inch below, &c.

In like manner, a line was drawn joining all those places where the thermometer stood at its mean height for the given hour and month; and this was called the line of mean temperature. Another line was drawn through all those places where the thermometer stood ten degrees *above* the mean; and other lines were drawn through the places where the thermometer was twenty degrees above the mean, ten degrees *below* the mean, twenty degrees below the mean, &c.

This mode of representing the observations of the barometer and thermometer appears to me the most suitable of any which has yet been proposed to indicate the connection between the pressure and temperature of the air on the one hand, and the direction of the wind and the fall of rain on the other.

The results of the investigation of the storm of Feb. 4, 1842, were entirely satisfactory to myself, and were published in Volume IX. of the Transactions of the American Philosophical Society. Being convinced of the superior advantages of this new method of investigating the phenomena of storms, I immediately constructed a series of maps upon the same principle, representing the progress of the storm of December, 1836; and I perceived that certain features of this storm were thus brought out more clearly than I had been able before to exhibit them; but, inasmuch as the publication of a series of colored maps involves a serious expense, these maps have never been offered for publication.

As, however, my paper upon this storm has been commented upon by Messrs. Redfield and Espy, and by Dr. Hare, and some conclusions have been drawn from it which appear to me unwarranted, I have thought it expedient to bring the subject before the notice of this Association.

The five accompanying maps represent the phenomena of this storm from the evening of December 19th to the evening of December 21st, at intervals of twelve hours, the mode of representation being similar to that already mentioned as having been employed for the storm of February, 1842.

The blue color represents the region where the sky was unclouded; the brown color represents the region where the sky was overcast, but without rain or snow; the fall of snow is indicated by the green color, and rain by the yellow. The line of mean pressure is drawn upon each map, as well as the line of two tenths of an inch below the mean. On the last three maps is also drawn the line of four tenths of an inch below the mean. In order to avoid confusion, the thermometrical lines have been omitted from these maps. As, on account of the dark color of the maps, the arrows indicating the direction of the wind could not be well seen at a distance, they have been omitted, and a separate chart has been prepared, showing the direction of the wind for the evening of December 20th.

I do not propose at present to undertake an analysis of the general phenomena of this storm; but shall confine my remarks to two points respecting which some diversity of opinion has been expressed.

The first question relates to the direction and velocity of the storm's progress. If our observations embraced the entire area of the storm for a series of days, it would be an easy matter to assign its direction and velocity, provided we can agree as to what shall be regarded as the centre of the storm. The progress of a rain-storm is generally characterized by a depression of the barometer; and perhaps there is no point to which we can more appropriately apply the term *centre of the storm*, than to that point where the depression of the barometer is greatest. Unfortunately, in the storm of December, 1836, few, if any, observations have been obtained from points north of the centre of the storm. The depression of the barometer at Quebec was greater than at any station south of it. But at Fort Snelling, near lat. 45°, on the Mississippi River, the depression of the barometer was no greater than it was at Natchez. It seems reasonable then to infer, that along the Mississippi River the point of greatest depression must have been near St. Louis; while near the Atlantic coast the centre could not have been south of Quebec. Assuming St. Louis and Quebec to lie in the central path, the direction of the storm's progress was from S. 62° W. to N. 62° E. It seems quite clear that the storm travelled towards some point north of east, and not towards the south of east, as has been inferred by Dr. Hare and by Mr. Espy. From the morning of December 20th to the morning of December 21st, the centre of the storm advanced 1,140 miles in the direction already assigned; being an average velocity of 48 miles per hour.

The second question which I propose to consider is, Was this storm a whirlwind? In order to answer this question, I have drawn, upon a separate sheet, arrows representing the direction of the wind for the evening of December 20th, being the period when the storm was most nearly central as regards the stations of observation. At this instant we find the winds in the rear of the storm to blow from the north or northwest. Their average direction is about N. 30° W.; and this mean direction is sensibly the same in lat. 45° as near the Gulf of Mexico.

In front of the storm, the winds generally blow from points between south and southeast. Its average direction is about S. 10° E.; and in the southern part of the United States the winds are quite as much easterly as in the northern part of the United States, and perhaps even more so. We thus find that along a meridian, for a distance of at least 1,200 miles, we have on the west side a violent current setting from N. 30° W.; and on the east side, in close proximity, an equally violent current setting from S. 10° E. The most striking feature exhibited by these winds is that of two currents blowing with great violence, for at least 48 hours, in directions almost diametrically opposed to each other. This is not a whirlwind, as that term is generally understood; nor according to the diagrams of Messrs. Redfield, Reid, and Piddington. It may be admitted, indeed, that the winds show some tendency to circulate in a direction contrary to the motion of the hands of a watch; but this tendency is not the predominant feature, and the centrifugal force thus resulting can produce only insignificant effects. But how can two winds blow thus violently towards each other for 48 successive hours? This southeast wind was confined to that portion of the atmosphere which is near the earth's surface. The northwest wind being colder, and therefore heavier, than the southeast wind, flowed under it, raised it from the earth's surface, and turned it back upon itself, so that at an elevation of two or three miles above the earth's surface a westerly wind was prevailing even in front of the storm. The air thus elevated from the earth's surface was chilled by the cold of elevation; its vapor was condensed, and was precipitated in the form of rain or snow.

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2. On the Storm of October 7, 1854, near the Coast of Japan, and the Conformity of its Progression with other Cyclones. By W. C. Redfield, of New York.

Since the return of the U. S. expedition from Japan I have obtained, through the kindness of Commodore Perry and his officers, some notices of gales encountered by the squadron while in the Pacific Ocean and the Asiatic seas.

One of these storms was encountered by the U. S. ships *Mississippi* and *Southampton*, on the 7th of October last, near lat. 35°, soon after leaving Japan on their way to the Sandwich Islands. On the 7th and 8th it was encountered by an American whaleship near lat. 45°; as appears by an extract from her log-book, for which I am indebted to Lieutenant Wm. L. Maury of the *Mississippi*. An important portion of its route, previous to and at the time of its recurvation, has also been brought to our knowledge by means of a report from Captain Briard, master of the English brig *Giffard*, then on her way from San Francisco to Shanghai, which is found in the London Nautical Magazine for February, 1855. Copies of these several reports are herewith submitted.

We have thus the necessary data for an approximate delineation of the route of this storm, which I have traced on the manuscript chart which is herewith presented. The track as thus shown may enable us to judge of its conformity with the general law of progression manifested by other cyclones, in different regions.

It will be seen that the change from a westerly to an easterly progression, in this cyclone, took place in 27° 30′ of north latitude, and eastward of the Loo Choo Islands. It is obvious that the geographical relations of its route to the equatorial zone and the northern hemisphere are not unlike those which have been shown in the storms of the North Atlantic Ocean.

In order to exhibit this accordance more perfectly, it has occurred to me to transfer a tracing of this storm-track to a chart of the North Atlantic, and one hundred and fifty degrees of longitude more eastward than its actual position, so as to interpolate it among the known tracks of the Atlantic storms. The track as thus shifted is marked in red ink on some copies of the chart which was prepared to illustrate my memoir on the Cape Verde and Hatteras hurricane and other storms, which was presented to the last meeting of the Association, at Washington.

Such comparisons of the great curves of atmospheric progression which are developed in these storm-tracks, will serve to show their remarkably persistent analogies; which clearly indicate that the same dynamical elements of rotation and progression are common to all these storms. This view appears confirmed by nearly all the storm-routes which hitherto have been established in different regions, and must claim the careful attention of meteorologists, as well as of all who take interest in the general physics of our globe.

It may be noticed, that I have also placed on the manuscript chart the trace of the *Raleigh's* typhoon of August, 1835, which passed over Macao and Canton. It is taken from my notice of that storm which was published in Silliman's Journal for January, 1835 (Vol. XXXV., first series). I have likewise added a tracing of the course of the Manilla typhoon of October, 1831, which was noticed in the same communication, and which is supposed to have extended its course to Balasore, on the Bay of Bengal. On the eastern portion of the chart will also befound p artial tracings of the storm-routes of the Pacific, near the coast of Mexico, which were noticed in my paper presented last year to the Association, at Washington; together with four storm-routes of the United States, Gulf of Mexico, and the Atlantic, taken from the storm-chart which accompanies that paper.

The field of the new chart extends from near Newfoundland and Barbadoes on the east, across the American continent and Pacific Ocean, to near the borders of British India on the west. Indeed, with the exception of the interior of the Asiatic continent, the field of successful inquiry may now be said to encircle the globe; and includes also the principal seas of the southern hemisphere.

B. CHEMISTRY AND NATURAL HISTORY.

I. CHEMISTRY.

1. Description of a New Apparatus for separating Gold and other Precious Metals from Foreign Substances. By E. N. Kent, of New York.

I invite your attention for a few moments to a brief description of a new apparatus for separating gold from foreign substances, which has been recently put into operation at the U. S. Assay Office in New York.

The principles involved in the construction of this apparatus are based on the following facts. Grains of gold, silver, or other ductile metals, when ground or crushed with quartz or other hard substances, flatten under the mill, and thus prevent the crushing surfaces from coming close enough to grind the stony matrix to a very fine powder. But if, when thus crushed as fine as possible, the grains of gold or other metal thus liberated are separated, the remaining stony matter can *then* be crushed to an impalpable powder, so that the finest particles of gold, and even such as are invisible to the naked eye, may be liberated, and with *proper* apparatus be saved.

To accomplish this great desideratum effectually, the mill *A* (reference to drawings,* fig. 1), and amalgamator *E*, are so constructed as to hold a large quantity of water, in each of which the earthy matters are kept constantly agitated and suspended, while the light and heavy substances are separated from each other by virtue of their respective gravities, the heavy metallic portions falling to the bottom of the columns of water, while the light and refuse earth is washed away in the current of water which constantly passes through the apparatus in the direction of the arrows (Fig. 2).

The earthy matters which have been previously crushed are supplied to the apparatus through the hopper *C*, in which the grains of gold or other precious metal (if any be present) will be retained, and cleaned by the action of the teeth on the under side of the plate *D* from the sand which would otherwise remain.

After the metallic grains have been thus separated, the earthy portion is carried by the current of water, through the tube (*r*), into and under the surface of the large body of water in the mill *A*, the light and finer portions passing off with the current of water into the amalgamator *E*, and the heavy and coarser portion falling to the bottom of the mill, where it is ground and mixed up with the water, until the earthy portion of this also passes off into the amalgamator, leaving a small residue very rich in gold at the bottom of the mill.

The earthy matters, from which the greater portion of the gold or silver has been now extracted by mechanical means, pass into the pan *H*, and are stirred up and suspended in the water by the teeth attached to the arms *I*, and are carried thence by the current of water,

* Not published.

through the small tubes (*l l*), into the second large body of water, contained in the vessel *E*. Here it is brought directly in contact with the mercury on the bottom, the surface of which is kept constantly clean by the revolving action of the paddle-wheels around the shaft (*k*), which also causes an outer current in the space between the wheels and the sides of the vessel, into which the earthy matters are thrown by the revolution of the paddle-wheels upon their own axis. The heavy metallic portion is thus allowed to fall by virtue of its gravity, and remain in contact with the clean mercury until amalgamated, while the light and refuse earth is rapidly washed away.

At the close of the operations for a day, the supply of water is stopped, and the cock (*d*) opened for a few minutes, while the shaft (*b*) is still in motion, so as to draw off the small rich residue which remains at the bottom of the mill into the pail *P*, while the water passes off through the overflow-pipe into the trough (*q*), and is conducted into the traps *K*.

The rich residue thus obtained from the mill contains, besides the precious metals, sand, sulphur, and generally iron. The two latter may be oxidized and removed by roasting or burning the residue, and by passing the ashes through the grain separator *C* at the next operation, either alone or with a fresh supply of earthy matter, the gold contained therein will be separated and cleaned. Or the residue may be fluxed with soda-ash and nitre, to remove the sand and iron, and the gold will be obtained at the bottom of the crucible.

The gold contained in the grain separator is removed through a tap at the bottom into a small pail, which for this purpose is to be hung on the hook (*n*). This is the largest and most valuable portion of the gold, and if properly cleaned by running the mill about two hours after the supply of earth has ceased, it is found to be clean and bright, and when dried is ready to be fused and cast into ingots.

The mercury in the amalgamator requires to be removed only once a week, or once a month, according to the quantity of material which has been passed through the apparatus. When sufficiently charged with gold or silver, the amalgam is to be drawn off through the cock (*i*) into the pail *P*, which retains it, and allows the water to pass off into the trap boxes *K*. The amalgam is then strained and distilled in the usual manner, and the fluid mercury returned to the amalgamator.

II. MINERALOGY.

1. On Some New Localities of Minerals. By Sanderson Smith, of New York.

At a copper mine about a mile from the celebrated barytes mine in Cheshire, Ct., I lately obtained some specimens of native silver, in cavities in massive copper glance. One of the filaments was about three quarters of an inch long, and the tenth of an inch in diameter. Associated with these, in cavities lined with minute quartz crystals, were a number of little plates about three tenths of an inch long by one fifteenth broad, and very thin. These were flexible, and generally curved, and were tinged of a beautiful green by copper. They may possibly be crystals of tremolite. A quantity of these fill the cavities, in which they are loose, not being attached together, or to the walls of the cavity. They are consequently difficult to collect and preserve, as they generally fall out in breaking the specimens open. This mine will probably soon be worked, when we may hope for interesting specimens.

In the city of New York, about 50th Street, crystallized muscovite, with internal hexagonal markings of a lighter color, occurs. When these markings are examined by polarized light, they present a different color from the groundwork, indicating a different angle between the axes of polarization.

At Reading, Pa., allanite occurs as the gangue of zircon.

The specimens labelled malachite, from Jones's Mines, Berks Co., Pa., are generally silicate of copper, instead of carbonate. The cerusite stated to occur there must, I think, be a mistake, as no lead minerals occur there. At Steel's Mines, in the same county, very good brown garnet is found.

At Cornwall, Lebanon Co., Pa., very handsome chrysocolla is found, as well as pretty strong loadstone, and a handsomely marked jasper.

At the zinc mines near Friedensville, Lehigh Co., Pa., have lately been found some specimens of crystallized carbonate of zinc (Smithsonite), in double-pointed pyramids, of about the size and shape of caraway-seeds, scattered over the surface of a thin film of velvety iron ore, investing massive carbonate of zinc. Some also occur directly on the massive carbonate, but those on the iron ore are larger, and make much more beautiful specimens, from the relief given by the dark ground. The crystals are much rounded, however, so that scarcely any faces can be made out. The crystallized lanthanite from this locality, described by Blake in the sixteenth volume of Silliman's Journal, is still unique. Openings are now being made at the spot where it was supposed to have been found, and we may hope that more will soon be discovered. At the allanite locality, near Bethlehem, small but very brilliant zircons occur, generally imbedded in the allanite. In some larger crystals, which are not so brilliant, the pyramidal faces are almost black, whilst the lateral faces are of a light buff color. Associated with the allanite and zircons are very acute rhombic prisms, sometimes more than an inch long, and three quarters of an inch in the longest diagonal, which are in a high state of internal alteration, though still preserving a tolerably bright surface and sharp edges. A blowpipe examination shows the presence of titanium, and the mineral will probably prove to be sphene, though no specimen exhibiting terminal planes has yet occurred. The basal cleavage is very perfect, but has the appearance of being the result of alteration, like that of the hydrated iolite.

About two miles north of Easton, Northampton Co., Pa., near the nephrite locality, some very handsome specimens of crystallized serpentine, having bright faces, and being very translucent, have been procured by Dr. Edward Swift, of Easton.

When at Chamouni last year I obtained a specimen marked "Mica Verte du Mont Rose," which has all the characters of Mr. Blake's clinochlore, and possesses a most beautiful green color, almost surpassing, I think, that from the original locality.

2. Re-examination of American Minerals. Part V. The Minerals of the Wheatley Mine in Pennsylvania. — Anglesite; Cerusite; Wulfenite; Vanadate of Lead; Pyromorphite; Mimetene; Galena; Copper; Copper Pyrites; Malachite; Azurite; Blende; Calamine; Hematite; Fluor Spar; Calc Spar; Sulphur, &c. By Professor J. Lawrence Smith, of Louisville.*

Before describing the minerals of this mine, it is well to say a word with reference to its location, and also to quote some remarks on the geology of the surrounding country by Professor H. D. Rogers. Although this is departing from the plan usually adopted, still the occurrence of all the minerals here described at one locality cannot but render the geology of the place interesting to mineralogists.

This mine is situated in Chester County, near Phœnixville, Pennsylvania, and is one of several interesting developments of a thorough and very able exploration of this region by Mr. Charles M. Wheatley. At the request of Mr. Wheatley, Professor Rogers made a geological examination of the metalliferous veins of this district, and the following remarks are taken from his report.

"These veins belong to a group of lead and copper-bearing lodes of a very interesting character, which form a metalliferous zone, that ranges in a general east and west direction across the Schuylkill River, near the lower stretches of the Perkiomen and Pickering Creeks in Montgomery and Chester Counties, and bids fair to constitute at no distant day a quite productive mineral region.

"The individual veins of this rather numerous group are remarkable for their general mutual parallelism, their average course being about N. 31° — 35° E. by compass, and not at all coincident with that of the belt of country which embraces them. They are true lodes or mineral injections, filling so many dislocations or fissures, transverse to the general direction of the strata which they intersect. The metalliferous belt ranges not far from the boundary which divides the gneissic or metamorphic rocks of Chester County from the middle secondary red shale and sandstone strata.

* The Association is indebted to Professor J. D. Dana for the figures and mathematical descriptions of the crystals given beyond.

" This vein varies in thickness from a few inches to about two and a half feet, and we may state its average width at not less than eighteen inches. It is bounded by regular and well-defined, nearly parallel walls, the prevailing material of which is a coarse, soft granite, composed chiefly of white feldspar and quartz.

" It would seem to be a pretty general fact, that such of these veins as are confined entirely or chiefly to the gneiss, bear *lead* as their principal metal; whereas those which are included solely within the red shale are characterized by containing the ores of copper. But the zinc ores, namely, zinc blende and calamine, prevail in greater or less portions in both sets of veins, existing, perhaps, in a rather larger relative amount in the copper-bearing lodes of the red shale.

" The *gneissic strata* of the tract embracing this group of lead-bearing veins seem to differ in no essential features from the rest of the formation ranging eastward and westward through this belt of country. Here, as elsewhere, they consist chiefly of soft thinly bedded micaceous gneiss, a more dense and ferruginous hornblendic gneiss, and, thirdly, a thicker-bedded granitic gneiss, composed not unfrequently of little else than the two minerals, quartz and feldspar.

" Penetrating this quite diversified formation are innumerable injections of various kinds of granite, greenstone trap, and other genuine igneous rocks. The granites, as throughout this region generally, consist for the most part of a coarse binary mixture of quartz and opaque white feldspar, tending easily to decomposition. This rock abounds in the form of dykes and veins, sometimes cutting the strata of gneiss nearly vertically, but often partially conforming with its planes of bedding for a limited space, and then branching through, or expiring in it in transverse or tortuous branches. A not uncommon variety of granitic dyke is a simple syenite composed of quartz, greenish semi-translucent feldspar, and a smaller proportion of dark green hornblende. A soft, white, and partially decomposed granite is a very frequent associate of the stronger lead-bearing veins, particularly in their more productive portions; but this material belongs, in all probability, not to the ancient granitic injections of the gneiss, but to those much later metalliferous intrusions which filled long parallel rents in that formation with the lead ores and their associated minerals.

" The gneissic strata and their granitic injections throughout this district display a softened, partially decomposed condition, extending in many places to a depth of twenty fathoms.

"Of the dozen or more lead and copper lodes of greater or less size brought to light in this quite limited region of five or six miles' length and two or three miles' breadth, the greater number are remarkably similar in their course, ranging N. 32° — 35° E., and S. 32° — 35° W.; and what is equally worthy of note, they dip, with scarcely an exception, towards the same quarter, or southeastwardly, though in some instances so steeply as to approach the perpendicular.

"There is no marked difference in the general character of the vein-stones of the several mineral lodes, nor any features to distinguish as a class those of the red shale from those of the gneiss."

The minerals found in these veins are quite numerous, and among them there are specimens of species hardly equalled by those coming from any other locality. Professor Silliman, in his report on the minerals of this mine exhibited at the Crystal Palace, says, that the specimens of sulphate and molybdate of lead are the most magnificent metallic salts ever obtained in lead-mining, and unequalled by anything to be seen in the cabinets of Europe.

48. *Anglesite.*

This mineral is found abundantly and in beautiful crystals at this locality. The magnificence of many of the specimens can only be realized by seeing those in Mr. Wheatley's cabinet. The crystals are remarkable for their size and transparency; in some instances they weigh nearly half a pound, being as transparent as rock-crystal in nearly every part. Crystals with terminations at both ends have been obtained five and a half inches in length by one and a half in thickness; perfectly limpid crystals an inch in length are quite common.

The following are some of the forms: —

1\. — 0, ∞, $1\text{-}\breve{\infty}$.

2\. — 0, $\frac{1}{2}\text{-}\bar{\infty}$, $\infty\text{-}\infty$, 1, ∞, $1\text{-}\breve{2}$, $1\text{-}\breve{\infty}$, $\infty\text{-}\breve{\infty}$.

3\. — 0, $\frac{1}{4}\text{-}\bar{\infty}$, $\frac{1}{2}\text{-}\bar{\infty}$, ∞, $1\text{-}\breve{2}$, $2\text{-}\breve{4}$.

4\. — 0, $\frac{1}{2}\text{-}\bar{\infty}$, $\infty\text{-}\bar{\infty}$, $\frac{3}{4}\text{-}\bar{\frac{3}{2}}$, 1, ∞, $1\text{-}\breve{2}$, $2\text{-}\breve{4}$, $1\text{-}\breve{\infty}$.

Sometimes the crystals of this mineral are full of cavities, and of a milk-white color; but these do not differ in composition from the colorless and transparent forms. It also occurs in circular crystals.

It is sometimes colored. There is a black variety produced by the more or less perfect admixture of the sulphurets of lead and copper (containing traces of silver) in the mass of the crystals, whose form is not altered. There are crystals of a delicate green color, arising from carbonate of copper, and others of a yellow color, due to oxide of iron.

The transparent and colorless variety is remarkably pure. Its sp. grav. is 6.35. On analysis it afforded

	1.	2.
Sulphuric acid,	26.78	26.61
Oxide of lead,	73.31	73.22
Silica,	.20	
	100.29	99.83

according very precisely with the formula $\dot{Pb}.\ \overline{\ddot{S}}$.

I would call attention to the method of analyzing this sulphate, as described in another paper, for it was analyzed in the moist way by dissolving it first in citrate of ammonia.

The anglesite of this mine is found variously associated. It is common to find it in geodic cavities in galena, the cavities being lined with hematite varying in thickness from $\frac{1}{20}$ to $\frac{1}{2}$ an inch or more, and often this hematite contains anglesite intimately mixed in the mass. It may occur in crystals occupying a portion of the geode, or it may fill its entire capacity, assuming the form of the cavity. It is also found compacted in the galena, without the appearance of any cavity, or the presence of any other mineral; acicular crystals occur diffused through the galena. Observed also on copper pyrites, with a thin layer of hematite intervening between the crystal and the pyrites,—on crystals of zincblende in quartz,—on quartz associated with pyromorphite,—on galena with crystals of sulphur,—on calc spar without any associate. One very interesting specimen consists of a flattened crystal an inch square, having a delicate crystal of calc spar, over an inch and a half in length, perforating the centre, and around which the sulphate appears to have formed. It is also found on fluor spar without associate.

Some of the most beautiful specimens are where large crystals of anglesite are covered with crystals of carbonate of lead, these latter frequently penetrating the anglesite.

49. *Cerusite.*

The crystals of this mineral, though not as large as those of anglesite, are yet exceedingly beautiful, both in size as well as transparency; the twin crystals are often two inches broad, transparent, and presenting the appearance of the spread wing of a butterfly; some of the single crystals are an inch in length, and half an inch thick.

A transparent crystal weighing five grammes gave a sp. grav. of 6.60, and on analysis furnished

Carbonic acid, ...	16.38	$\} = \dot{Pb}\,\ddot{C}.$
Oxide of lead, ...	83.76	
	100.14	

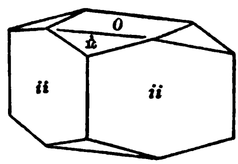
It occurs in hematite coating galena, in a manner similar to the anglesite, and associated with it; also in connection with pyromorphite, which often colors the entire body of the crystals of cerusite. It is found on galena without the association of any other mineral, — on green and blue carbonate of copper, — on pyromorphite, which often covers the entire surface of the cerusite crystals, imparting to them an opaque yellowish-green color, — on oxide of manganese, in snow-white crystals, without any other associate, — on hematite in a similar manner; mammillary masses of the hematite sometimes pass through the crystals. Some few specimens have been found consisting of crystals of galena, with a number of very fine hemitrope crystals of cerusite on the surface. The cerusite is occasionally covered with an exceedingly thin coat of oxide of iron, giving the crystals a dark red appearance; and some of them, again, with a very thin layer of pyromorphite, as delicate as if it had been put on with a brush.

The cerusite is sometimes colored black, green, and yellow, in a manner similar to that mentioned under anglesite.

50. *Wulfenite.*

This mineral is found in small crystals of every shade of color, from a light yellow to a bright red; it has been found in some abundance, forming, from the manner of its occurrence, very beautiful specimens. The crystals present a variety of modified forms, tabular and octahedral, one of which is here figured.

1.



Other forms are 0, 1.

0, $\frac{1}{3}$, 1.

0, $\frac{1}{16}$, ∞-∞. (Fig. 1.)

Specific gravity of a dark yellow variety, 6.95.

The composition of both the yellow and red varieties was examined; the difference of color is due to the presence of *vanadic acid* in the red varieties, and the intensity of color is proportional to the amount of vanadic acid, which in no instance is much more than one per cent.

The analyses afforded

	Yellow variety.	Red variety.
Molybdic acid, ...	38.68	37.47
Vanadic acid, ...	——	1.28
Oxide of lead, ...	60.48	60.30
	99.16	99.05

The second corresponds very nearly to 97 per cent of molybdate, and 3 per cent of vanadate of lead. As the last substance varies in quantity, it cannot be regarded as giving a distinct specific character to the mineral. This mineral has been described as a chromo-molybdate of lead, but by the most careful examination only a trace of chromium can be detected; in fact, the quantity is so minute as to require further examination in larger quantities to place the matter beyond a doubt.

Wulfenite occurs alone on crystallized and cellular quartz, or associated with pyromorphite, whose beautiful green color is often very much enhanced by the contrast of the yellow and red crystals on its surface.

Sometimes the wulfenite forms the mass, and crystals of pyromorphite are sparsely disseminated over the surface. It is also found in decomposed granite, — on carbonate of lead and oxide of manganese, — also associated with vanadate of lead.

51. *Vanadate of Lead* (*Descloizite?*).

This species has never before been remarked among American minerals, although the chloro-vanadate (vanadinite) was first discovered in Mexico. This adds another to the list of curious minerals from the Wheatley mine. It was noticed about a year ago in the form of a dark-colored crystalline crust, covering the surface of some specimens of quartz and ferruginous clay, associated with other minerals. Observed with a magnifying-glass, it is seen to consist principally of minute lenticular crystals, grouped together in small botryoidal masses; the crystalline structure is perfect. Thus seen, the color of the mass is of a dark purple, almost black. When seen by transmitted light, the color is dark hyacinth-red, and translucent. The streak is dark yellow. From the difficulty of obtaining any quantity of sufficient purity, nothing accurate can be stated with reference to its specific gravity and hardness; and for the purpose of analysis I was obliged to use material which, although containing pure crystals of the vanadate, was yet mixed with crystals of molybdate of lead, and other impurities.

The chemical analysis is an imperfect one, yet the best that can be made from the mineral as it has been found. It is as follows: —

Vanadic acid,	11.70
Molybdic acid,	20.14
Oxide of lead,	55.01
Oxides of iron and manganese, Alumina,	5.90
Oxide of copper,	1.13
Sand,	2.21
Water,	2.94
	99.03

If we subtract the amount of oxide of lead requisite to form wulfenite with the molybdic acid present, we have left 22.82 per cent, which is combined with 11.7 of vanadic acid, making a compound corresponding to, Vanadic acid 66.1, oxide of lead 33.9 = 100.

This result is not considered precise; it corresponds, however, more nearly with the composition of descloizite, as given by Damour, ($\dot{P}b^2 \ddot{V} = \ddot{V}$ 29.3, $\dot{P}b$ . . . 70.7,) than with dechenite, by Bergmann ($\dot{P}b \ddot{V} = \ddot{V}$ 45.34 $\dot{P}b$ 54.66).

The composition of Descloizite cannot be considered as having been fairly made out, for Damour's results are deduced, as mine have been, from a very impure material, and may on future examination prove to be $\dot{P}b^3 \ddot{V}^2$; corresponding in composition to the chromate of lead called melanochroite. This mineral has as yet been found only in small quantity at this mine, associated with oxide of manganese and wulfenite, the crystals of this latter substance being more or less covered with minute crystals of the vanadate.

52. *Pyromorphite.*

There are several shades of color belonging to this mineral,— a green so dark as to be almost black, olive-green, pea-green, leek-green, greenish-yellow, and all intermediate shades. It is a very abundant ore at the Wheatley mine, and large quantities of it are smelted. Specimens of great beauty are found occurring in botryoidal masses with columnar structure, in perfect hexagonal prisms with the summits more or less modified. Crystals are found half an inch in diameter. Some of the crystals are hollow, with only an hexagonal shell; sometimes the crystals are agglomerated in a plumose form.

A dark green variety gave a specific gravity of 6.94. No analysis was made of this mineral, as it will be embraced in an examination of the American pyromorphites, to be published at some future time.

It is found in decomposed granite, on quartz crystals, occasionally covering their entire surface; in cellular quartz, with molybdate of lead; in large masses of grouped crystals, with small crystals of yellow and red molybdate inserted on crystals of sulphate and carbonate of lead, and forming a coating to large surfaces of galena.

53. *Mimetene.*

The specimens of this mineral that have been found, although few in number, are remarkable for their beauty of crystallization. Some of the crystals are nearly colorless, and perfectly transparent; others of a lemon-yellow, either pure or tinged with green. The form is that of a perfect hexagonal prism, the edges of the summit most commonly truncated, often to such an extent as to terminate the crystal with an hexagonal pyramid (Fig. 2). The crystals are sometimes as small as a hair, and a quarter of an inch, or more, in length; and again they are so broad and short, as to form hexagonal plates half an inch across.

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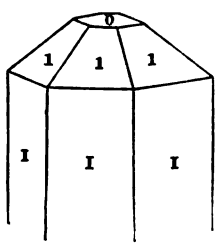

A specimen of the lemon-yellow variety was examined; it gave a specific gravity of 7.32, and was found to contain

Arsenic acid,	23.17
Chlorine,	2.39
Oxide of lead,	67.05
Lead,	6.99
Phosphoric acid,	.14
	99.74

corresponding to, Arsenate of lead 90.66, chloride of lead 9.34 = $\dot{Pb}^3 \ddot{\ddot{A}}s + \frac{1}{3}$ Pb Cl.

This specimen of mimetene is seen to be almost free from phosphoric acid, containing only about $\frac{1}{10}$ of one per cent, in this respect resembling that from Zacatecas, as analyzed by Bergmann.

This mineral is found in granite or quartz. It is also associated with pyromorphite, and sometimes the two run together, so as to present no distinct line of demarcation between them; some of the specimens consist of the two minerals, the pyromorphite forming one entire surface, and mimetene the opposite surface, and between, various shades of the mixture. It has been found with galena and carbonate of lead.

54. *Galena.*

The compact, fibrous, and crystallized varieties of galena occur at this mine. Fine crystals are found, either a perfect cube or a cube with modified edges and angles, octahedron and rhombic dodecahedron, often very much flattened out, and occasionally rounded to an almost globular form; these rounded crystals are usually covered with pyromorphite. The galena is sometimes cellular, arising from partial decomposition, the exterior portion presenting a black drusy appearance, the interior of a bright steel color; this variety is particularly rich in silver, and also contains crystals of sulphur.

The galena is argentiferous, giving an average yield of thirty ounces to the ton. It is found associated with quartz, calcite, and fluor spar, frequently inserted in the crystals of these substances; it is also a common associate of all the minerals of this locality. Some of the cubical crystals have their surfaces partly decomposed, and covered with a layer of crystals of carbonate. Specimens are found of very large cubical and octahedral crystals, forming slabs several square feet in surface, completely covered with a layer of leek-green phosphate. The cavities of the galena frequently contain sulphur.

55. *Copper.*

Native copper is found only in delicate films on hematite or quartz crystal, and forms an interposing layer between the hematite and copper pyrites.

56. *Copper Pyrites.*

Copper pyrites is found in some cases in sufficient quantity to be worked as an ore; some of the masses are of considerable size, weighing three or four hundred pounds. Fine crystals are obtained, both tetrahedral and octahedral. It affords on analysis,

Sulphur,	36.10
Copper,	32.85
Iron,	29.93
Lead,	.35
	99.23

It occurs alone, and associated with the other sulphurets. It is found in various parts of the vein, there being no special point of deposit.

57. *Malachite.*

Malachite occurs in small reniform masses, consisting of fibrous crystals, and of a bright green color; also in silky tufts of a very light green color, which are associated with azurite and carbonate of lead. Its specific gravity is 4.06. An analysis gave,

Carbonic acid,	19.09
Oxide of copper,	71.46
Water,	9.02
Oxide of iron,	.12
	99.69

affording the formula $\dot{C}u\,\ddot{C} + \dot{C}u\,\dot{H}$.

It is associated with the various ores of copper and lead of the Wheatley mine, and sometimes so thoroughly diffused through the sulphate and carbonate of lead, as to give them a uniform green tint. It is not found in any quantity.

58. *Azurite.*

This mineral, although rare, is found in beautiful crystals, some measuring from $\frac{1}{4}$ to $\frac{1}{2}$ inch across, of a deep blue color, and highly polished faces. Its specific gravity is 3.88. An analysis gave,

Carbonic acid,	24.98
Oxide of copper,	69.41
Water,	5.84
	100.23

giving the formula $2\,\dot{Cu}\,\ddot{C} + \dot{Cu}\,\dot{H}$.

This species occurs in similar associations with the malachite. It is however rarer.

59. *Zinc Blende.*

Blende is found in considerable quantity, both massive and crystallized. Some of the crystals are exceedingly beautiful, and of large size, being three or four inches in diameter, and with very brilliant surfaces. The colors are dark hair-brown and black, the brown being transparent. The specimens from this locality are hardly surpassed by those from any other mine. A specimen that was analyzed gave the following results: —

Sulphur,	33.82
Zinc,	64.39
Cadmium,	.98
Copper,	.32
Lead,	.78
	100.29

It is proposed to examine yet other specimens, to see if there may not be larger amounts of cadmium contained in some of them.

This mineral occurs in fluor spar, calc spar, and quartz, more or less mixed with the other sulphurets. In some instances it is very peculiarly interlaced in the rocks; thus we have specimens consisting as it were of four layers; namely, granite, then compact crystallized quartz three fourths of an inch thick, then the blende an inch thick, on that a layer of crystals of calc spar, and on this last fluor spar.

60. *Calamine.*

Calamine is found in delicate crystals of a silky lustre, forming in some instances snow-white tufts on fluor spar, blende, and carbonate of lime. It is also found on cellular quartz. Some of the specimens are quite handsome, having a blue and yellow color from the presence of carbonate of copper and oxide of iron. No analysis was made of any of the specimens.

61. *Brown Hematite.*

This ore occurs in concretionary masses, of a dark liver-color and compact structure, associated with nearly all the minerals of this mine. It very commonly forms a lining to cavities in galena, in which are found crystals of anglesite and cerusite; sometimes it lines cavities in the rock that are completely filled with cubical galena. Acicular concretions of the hematite are found traversing crystals of anglesite and cerusite. A specimen of the purest hematite gave for its composition,

Peroxide of iron,	80.32
Oxide of copper,	.94
Oxide of lead,	1.51
Water,	14.02
Silica,	3.42
	100.21

62. *Fluor Spar.*

The remarkable feature of the fluor spar of this mine is the absence of color, all the specimens yet found being colorless and transparent. The crystals are very perfect and beautiful, yet small; it is sometimes in globular concretions, of crystalline structure radiating from the centre. The cube, which is the more common crystalline form, is sometimes very much modified by the truncation of the edges and angles. A specimen that was examined gave a specific gravity of 3.15, and the following composition: —

Fluorine,	48.29
Calcium,	50.81
Phosphate of lime,	*trace.*
	99.10

It is associated with calc spar, and in some instances in a remarkable manner, mentioned under the head of calc spar. Galena and blende are interspersed through it. Its occurrence in the mine was first noticed at the depth of three hundred feet, and since then it has been found abundantly.

63. *Calc Spar.*

There are a variety of interesting forms and associations of this mineral. The two most common are the dog-tooth spar and the hexagonal prism with a three-sided summit, and occasionally the hexagonal prism with flattened summits like arragonite. Sometimes slabs of this mineral are found, with a surface of eight or ten square feet completely covered with prismatic crystals an inch or two in length, and from half an inch to an inch in thickness; they are mostly vertical, but occasionally horizontal, with double terminations. These crystals are sometimes of a remarkable character, being eight or ten inches in length, and only a quarter of an inch in diameter, preserving a tolerably perfect hexagonal shape throughout the entire length; again, these slender forms are built up of small hexagonal prisms, their faces projecting from the side. It sometimes happens that these slender crystals are crossed by one of the same diameter, and less length, firmly attached in the manner of a cross.

But of all remarkable crystallizations is one where the small prisms are so arranged as to form a perfect double spiral arranged around an axis (Fig. 3); the specimen is three inches in length, and three

3.

eighths of an inch in diameter, with the space of a fourth of an inch between each turn of the spiral. The spiral arises from one small prism, crossing another at middle at a small angle of divergence (40° – 50°), and so on in succession. These slender crystals are sometimes curved in a very remarkable manner.

Another thing to be remarked in connection with the calcite of this mine is its singular associations; thus, we find groups of hexagonal prisms where a small cubical crystal of fluor, about the twentieth of an inch, is inserted in a small pit in the summit of almost every crystal (Fig. 4), without the occurrence of fluor spar on any other parts of the crystal. These crystals appear to have been formed

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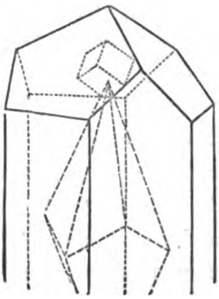

by successive crystallizations. Dog-tooth spar seems to have been first formed with these small crystals of fluor spar on their extremities, and then by a subsequent process the calcite has closed around the dog-tooth spar in the form of an hexagonal prism with a three-sided summit. The summit never closes entirely at the centre, the fluor spar remaining visible on one side, and where there is no crystal of fluor spar, the extremity of the dog-tooth spar is frequently seen.

Other groups of calcite crystals have minute crystals of iron pyrites in the three faces of the summit, arranged near and perfectly

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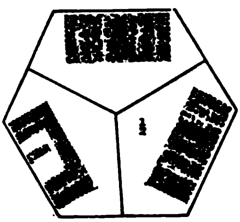

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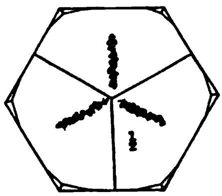

parallel to the alternate edges, as seen in Fig. 5. Every crystal in the group is thus furnished with a set of crystals of pyrites.

In another group of crystals, the pyrites, in equally small crystals, is found in three lines on the summit of every crystal, running from the apex towards the edges, exactly bisecting each face, as seen in Fig. 6.

In this instance, as well as in the former, the pyrites is inserted entirely beneath the surface of the crystal, which is perfectly smooth.

The calcite is found in large crystals in dolomite, and is associated with most of the ores of the mine. It sometimes gives rise to pseudomorphs of molybdate of lead and carbonate of lead; these pseudomorphs are mere shells, however, retaining the form of the calcite.

64. *Sulphur.*

Sulphur occurs in the form of small pale greenish-yellow crystals; they are transparent, and disseminated through cellular galena, which appears to have undergone partial decomposition; the galena in which it occurs is frequently associated with copper and iron pyrites, and in some rare instances with carbonate and phosphate of lead.

The other minerals occurring in the Wheatley mine are finely crystallized *quartz*, *oxide of manganese*, *iron pyrites*, *sulphate of baryta*, *indigo copper*, *black oxide of copper*, and *dolomite*.

Of the other mineral veins in this region, none have yielded the beautiful mineral species furnished by the Wheatley vein. The Perkiomen vein, five miles from the Wheatley vein, has furnished fine *capillary copper*, *indigo copper*, fine acicular crystals of *sulphate of baryta*, *crystallized copper*, and some crystals of *sulphate*, *carbonate*, and yellow *molybdate of lead;* but these last were small, and bear no comparison to those described.

It was hoped that something might be learned concerning the formation of the minerals of this vein, but the difficulties and uncertainty attendant upon the study of questions of this kind make it prudent to postpone any views that might be suggested. It may, however, be well to remark, that, in opening the vein, and descending from the surface for the first thirty feet, the phosphate of lead was very abundant, with some galena and carbonate; a little lower down, the phosphate was less, and the carbonate more abundant. Wulfenite and anglesite began to appear at 120 feet; the phosphate and carbonate still continued with the galena, with fine large crystals of anglesite, and considerable wulfenite; at 180 feet, phosphate very much diminished, carbonate and sulphate in fine crystals; arsenate was found here; at 240 feet, blende, calamine, and fluor spar appear, with considerable dolomite, and but little phosphate of lead, galena forming almost the whole lead ore; anglesite is found, but in smaller crystals. These observations may hereafter lead to some conclusions as to the manner of the formation of these minerals, but at present I prefer dismissing the subject without further remarks.

III. GEOLOGY.

1. Remarks on some Points connected with the Geology of the North Shore of Lake Superior. By J. D. Whitney.

(*Abstract.*)

The object of this communication was to give a sketch of the results of observations made by the author on the north shores of Lake

Superior and Lake Huron, during a portion of two seasons after the completion of the geological survey, by Mr. Foster and himself, of that part of the southern shore which is included in the Lake Superior Land District. Apart from the scientific interest attaching to an examination of the northern shore of this lake, it became a matter of great practical importance that the analogy between the rocks of the two sides of the lake should be studied, with reference to the probable value of the cupriferous veins which were known to exist, and which have been more or less extensively worked at various points on the Canadian side of Lake Superior, but nowhere with that success with which they have been, and are now, wrought within the territory of the United States.

The northeastern side of the lake, from Gros Cap, near its outlet, to the northeastern corner of Neepigon Bay, is formed by rocks of the azoic system. Occasional outliers and detached portions of the Lake Superior sandstone, forming the base of the palæozoic system of the Northwest, lie along the shore; and most of the large islands at this end of the lake, with the exception of Michipicoten, show nearly horizontal strata of the same rock, indicating that the basin of the lake in this direction was once covered with sandstone, which has since been removed by denudation. Thus the lake basin may be divided into two portions by a line drawn from Keweenaw Point to Michipicoten Island; that to the north owes its form to the existence of an axis of depression between two parallel lines of elevation, running nearly northeast and southwest, and from 50 to 60 miles distant from each other; while that portion which lies to the south has been simply scooped out by the action of water. The considerably diminished depth of the southeastern part of the lake, as compared with the northern and central portions, would appear to confirm this opinion, although the soundings which have been taken as yet are not sufficiently numerous to show the exact shape of the bottom of the lake.

From Saut Ste. Marie, following the south shore in its whole extent, and along the north side as far east as the northeastern extremity of Neepigon Bay, we find exposed on the lake shore only shales, sandstones, and conglomerates, the equivalent of the Potsdam sandstone, and the accompanying trappean rocks, with the exception of the north side of Thunder Bay and the vicinity of Carp River, where the lake has its extreme southern extension. At each of these points the sandstone has been washed away, so as to allow the azoic to make its appearance along the lake shore for a distance of a few miles. An almost straight line drawn northeast and southwest from the northeast corner of Neepigon Bay, along the north side of Thunder Bay, crossing the Kamanistiquia River at Kakabeka Falls, and Pigeon River at the falls of the same name, and keeping nearly parallel with, and a few miles distant from, the coast of the lake from this point westward, marks the southern limit of the azoic on the north side of the lake. An inspection of the map of Lake Superior will show how marked has been the influence of this line of upheaval in determining the general outline of the lake on this side. On the south side the northern line of the azoic still preserves an approximate parallelism with the shore; but a belt of sandstone and trap much wider than that on the other side interposes between it and the lake, and the axis of depression becomes less and less marked towards the west.

Similar as are the north and south shores of the lake in the general features of their geology, the details of the structure which they exhibit are very different. The most important distinction between the two shores is in the character of the igneous masses which accompany the sedimentary rocks. On the south side we have gently curving bands of trappean rock, of quite regular thickness and persistent in lithological character, which usually extend for many miles uninterruptedly. Thus, the great greenstone ridge of Keweenaw Point may be followed for more than forty miles, forming the most conspicuous feature in the topography of the region, and everywhere underlaid by a band of conglomerate, which, though only a few feet in thickness, extends with perfect regularity from the extremity of the Point to beyond the Cliff Mine. There are but few cross-fractures, slides, or heaves of the beds of rock upon this side of the lake, and no dykes whatever from one end of the region to the other. On the other side, the change in this respect in the character of the igneous rocks is most marked. Isle Royale, lying from 15 to 20 miles in advance of the entrance to Thunder Bay, is mostly made up of bedded traps, and is the counterpart of the north side of Keweenaw Point. The beds of rock are, however, usually thin, and very varying in lithological character, and the same is true of Michipicoten Island, although to a somewhat less extent. That part of the northern coast which belongs to the Territory of Minnesota is quite regular in its general outline, and has but few islands in advance of it; it is made up of bands of slates and sandstones, which are often much hardened and changed in character, and which alternate with bedded traps and volcanic grits. The whole system has a dip towards the lake, as on the south side. The great number of dykes which intersect the bedded traps and sandstones in this region form a marked feature in its geology, and one which differs from anything exhibited on the southern shore. The beds of rock being thin, and greatly disturbed by the dykes, are with difficulty traceable for any considerable distance.

As we approach Pigeon River, which forms the boundary between Minnesota and Canada, and which is nearly opposite the western end of Isle Royale, the shore of the lake becomes very irregular in its outline, and deeply indented by bays, and forms lofty and precipitous cliffs, which display some of the most picturesque and striking scenery of the Northwest. Between Pigeon River and Fort William the region bordering on the lake is exceedingly broken, and rises in precipitous ranges, sometimes to the height of 1,000 feet above the lake. They have a trend rudely parallel with that of the coast, but are very irregular in their outline. The rock of which they are composed is a trap, usually hard and crystalline, resembling that of the southern range of Keweenaw Point. It is frequently traversed by vertical planes of cleavage, so as to assume a columnar structure, and occasionally it takes on the appearance of a trap shale, being readily separated into flat plates or laminæ by horizontal lines of division. McKay's Mountain and Pie Island are two of the most conspicuous landmarks of this region. Thunder Cape is the most elevated point on the shores of Lake Superior, and one of the most interesting. It consists of a narrow ridge from four to five miles in length, elevated in its highest point about 1,350 feet above the lake. Very thinly bedded slates or shales of a dark color, and somewhat argillaceous, which sometimes divide into sheets as thin as paper, make up nearly 800 feet of the thickness of this mass. These slates, which lie nearly horizontally, are capped by a sheet of trappean rock from 200 to 300 feet in thickness. This elevation occupies the extremity of the neck of land, which runs in a northwest and southeast direction, and separates Black from Thunder Bay. A few miles east of the extremity of the point of Thunder Cape, the rock exposed along the shore is sandstone, which dips from 10° to 15° to the southeast, and is made up of buff and red layers, which are rather thinly bedded. The dark slates of Thunder Cape seem to be only a local modification of the sandstone, similar to that which occurs at Black and Presqu' Isle Rivers on the south shore, where a dark-colored, highly fissile slate passes gradually into the usual light-colored, heavy-bedded sandstone of the region. In the isolated precipitous knobs and ridges of the trap in this region we see the evidence of very considerable displacements of the rocks along lines of fracture, extending nearly northeast and southwest, and of secondary shorter lines at right angles to this direction. There are also marks of powerful denudation, and to this cause, as well as to the shortness and abruptness of the lines of elevation, the broken and irregular form of the coast must be ascribed. The peninsulas which separate Thunder Bay from Black Bay, and the last named from Neepigon Bay, are low, and show strata of sandstone dipping gently to the southeast. If the surface of the lake were elevated a few feet, probably not more than a hundred, these bays would all become connected with each other, and this part of the lake shore would extend, in a nearly regular line, from northeast to southwest, having in front of it, at a distance of 10 or 15 miles, a range of lofty and precipitous islands, of which what is now Thunder Cape would be the most conspicuous.

The distance to which the trap and sandstone formation extends towards the northeast from the northern extremity of Neepigon Bay is not known. A few miles up Neepigon River, which runs into the bay at its northwest angle, cliffs of trap, from 700 to 800 feet high, may be observed resting on beds of red and somewhat argillaceous sandstone. From the summit of these cliffs, looking to the northeast, the same kind of formation seems to extend to a considerable distance. St. Ignace, Simpson's, and the Vein Islands, which lie in front of Neepigon Bay, are made up of alternating beds of trappean rock and sandstone, with some conglomerate, and resemble Isle Royale in their lithological character.

Veins have been opened and worked on the east side of the lake in the azoic, and on the north side in the trap accompanying the Lake Superior sandstone, and numerous indications of copper have been observed at various points along the whole line of the shore. So far as is known, there are no mines now in operation on the Canada side, nor have there ever been within the borders of Minnesota. On Lake Huron, however, mining has been and is now carried on to a considerable extent in the azoic, with fair prospects of success.

As a general rule the following is true of the metalliferous veins on both sides of the lake. In the azoic the veins have purely quartzose gangues, and bear copper pyrites and the sulphurets of copper, with some sulphuret of iron. In the unbedded traps, which have been protruded among the alternating beds of trap and sandstone, as in the south range of Keweenaw Point and the ranges northwest of Isle Royale, the ores are sulphurets of copper and copper pyrites, with considerable sulphuret of zinc, and occasionally native silver, while the gangues are chiefly calcareous spar and heavy spar. Of the latter mineral there are numerous heavy veins; but when either this veinstone or calcareous spar occurs unmixed with quartzose matter, they rarely carry any traces of ore. In the bedded trappean rocks and the interstratified conglomerates, copper and silver occur almost solely in the native state, and are accompanied by zeolitic veinstones mixed with quartz and calc spar. And it is this latter class of veins only in which large and profitable mines have thus far been wrought. In these, regularity and thickness of the beds of igneous rock seem to have been necessary for the formation of largely productive and well-developed veins. When the beds of rock are thin, rapidly changing their lithological character, brecciated in their structure, or deranged by irregular upheaval and frequent cross-fractures, the veins have been rarely found rich in metallic contents. In the unbedded trap, which is frequently very crystalline, and has sometimes a porphyritic structure, the veins are sometimes very wide and regular, but the ore is always very sparsely disseminated through the veinstone, and they have not, in any instance, been worked with profit.

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2. On the Occurrence of the Ores of Iron in the Azoic System. By J. D. Whitney.

The object of the present communication is to call attention to the geological position and mode of occurrence of one of the most interesting and important classes of the ores of iron, namely, those which are associated with rocks of the azoic system.

The term Azoic, first applied by Murchison and De Verneuil in their description of the geology of the Scandinavian Peninsula, has been adopted by Mr. Foster and myself in our Reports on the Geology of the Lake Superior Land District, and has been shown by us to be applied with propriety to a series of rocks which covers an immense space in the Northwest. We have called attention to the fact, that this system of rocks, wherever it has been demonstrated to exist, has been found characterized by the presence of deposits of the ores of iron, developed on a scale of magnitude unlike anything which occurs in any of the succeeding geological groups or systems of rocks.

As illustrations of these views, we have briefly described some of the great ferriferous districts of the world, and particularly those of Lake Superior, Scandinavia, Missouri, and Northern New York, all of which exhibit the most marked analogy with each other, both in regard to the mode of occurrence and the geological position of the ores. The two last-named regions, however, not having been thoroughly examined by us in person, we were obliged to content ourselves with information obtained from others, in making a comparison of their most striking features.

Strongly impressed with the interest attaching to this subject, I availed myself of the first opportunity, after the publication of our Report, to visit the iron regions of Missouri and Northern New York, from the last-named of which I have just returned, after a careful examination of the most important localities where ore is now mined in that district. While it is intended to take another opportunity for giving a minute and detailed account of this region, it may be permitted to recapitulate here the principal points maintained by Mr. Foster and myself, and to the general correctness of which my more recent explorations have furnished me with additional evidence.

We maintain, therefore, —

1. That deposits of the ores of iron exist in various parts of the world, which in extent and magnitude are so extraordinary as to form a class by themselves. The iron regions mentioned above offer the most striking examples of the deposits now referred to.

2. That the ores thus occurring have the same general character, both mineralogically and in their mode of occurrence, or their relations of position to the adjacent rocks.

3. That these deposits all belong to one geological position, and are characteristic of it.

The extent of the workable deposits of the ores of the useful metals is usually quite limited. Most of the veins which are wrought in mines throughout the world are but a few feet in width, often not more than a few inches. This is true of the ores occurring in veins. In sedimentary metalliferous deposits, such as those of the ores of iron in the carboniferous, the horizontal extent is often very considerable; but the vertical range is so limited, that the most extensive basins may be in time exhausted, when worked on so extensive a scale as is the case in some of the celebrated iron districts of Great Britain. The deposits of iron in the azoic, however, are many of them developed on such a scale of magnitude, that the term " mountain masses " may be applied to them without exaggeration, while, from the very nature of their occurrence, they must extend indefinitely downwards, and cannot be exhausted. Thus the great iron mountain of Gellivara, in Sweden, has a length of three or four miles, and a width of not less than a mile and a half. Of course such a mass of ore, without limit in depth, might be worked on the most enlarged scale for any length of time without fear of exhaustion. The same may be said of some of the iron knobs and ridges of Lake Superior and of Missouri. They form veritable mountains of ore, and ages must elapse before their dimensions will have been perceptibly diminished. This is not necessarily the case with all the localities of ore of these districts. Indeed, in Northern New York and in Scandinavia, although there are accumulations of iron which may be measured by hundreds of feet, or even by miles, yet those which are best known and most worked are of much more reasonable dimensions.

The character of the ores thus occurring is mineralogically peculiar. They consist uniformly of the oxides, either the magnetic or the specular. Hydrous ores, carbonates, and the like are altogether wanting, unless it be upon the borders of the ore deposits, where a secondary metamorphic action between the ferriferous mass and the adjacent rocks may have taken place. The oxides found in this geological position are in general remarkably free from all injurious substances, such as sulphur, arsenic, lead, or zinc, and usually the approach to chemical purity in the ores is in proportion to the extent of the mass, the largest deposits being the purest. The principal foreign ingredient mixed with these ores is silica, which is always present, although frequently in minute quantity. Indeed, the analyses of the Lake Superior and Missouri ores show, in some instances, a surprisingly near approach to a state of absolute purity. It would not be difficult in some localities to procure large quantities of an ore not containing more than two or three tenths of one per cent of foreign matter, and that exclusively silica. The purity of the ores may be inferred from the high character and value of the iron manufactured from them when they have been skilfully worked, as, for instance, in Sweden. Some samples of iron manufactured from Lake Superior ore have, when tested, exhibited a degree of tenacity unequalled by that from any other part of the world. The ores of Lake Superior and Missouri are mostly peroxides; those of Northern New York almost exclusively magnetic; while in Scandinavia the magnetic and specular ores are both of frequent occurrence. Those of New York, on the one hand, are often coarse-grained and highly crystalline, while the peroxides of Lake Superior and Missouri are rarely distinctly crystallized, but are very compact.

The mode of occurrence of these ores in the regions above mentioned is so peculiar, that, from this point of view alone, it is apparent that these deposits should be classed together as distinct from those in the later geological formations. In all the characteristics of true veins, the great masses of ore now under consideration are wholly wanting. Some of the least important of them approach much nearer to segregated veins, and might with propriety be classed with them, were they not developed on so large a scale as to render it difficult to conceive of segregation as a sufficient cause for their production.

In the case of the most prominent masses of ore of these regions, there is but one hypothesis which will explain their vast extent and peculiar character. They are simply parts of the rocky crust of the earth, and, like other igneous rocks, have been poured forth from the interior in the molten or plastic state. No other origin can be assigned to the dome-shaped and conical masses of Lake Superior and Missouri, or to the elongated ridges of the first-named region. The Iron Mountain of Missouri forms a flattened dome-shaped elevation, whose base covers a surface of a little less than a square mile, and which rises to a height of 200 feet above the general level of the adjacent country. The surface of the mountain, when uncovered of the soil, is found to be covered with loose blocks of peroxide of iron, without any admixture of rocky pebbles or fragments, which are found to increase in size in ascending to the summit, where large blocks of ore of many tons in weight lie scattered about, and piled upon each other. It is a most singular fact, that the ore is nowhere seen in place about the mountain, although the whole mass evidently consists of nothing else. Near its base, an excavation seventeen feet deep has been made, which exhibits nothing but small, somewhat rounded fragments of ore closely compacted together, without any other substance being present except a little red, ferruginous clay, which seems to have been formed by the friction of the masses against each other. This feature in the Iron Mountain is one of peculiar interest, and one which it seems difficult to explain. Evidences of drift action in this region are exceedingly faint. The ore itself is one which seems little likely to undergo decomposition from any exposure to atmospheric changes. The blocks upon the summit, although somewhat moss-grown, have their angles and edges but little rounded. As a key to the origin of the ore, we find in close proximity on the north a long elevation of a reddish porphyry of unmistakably eruptive character, connected with the Iron Mountain by a narrow ridge of a rock composed of iron ore and feldspathic rock, showing that the porphyritic ridge and the ore-mass must have originated at one and the same time, and in the same way.

The eruptive origin of the great Lake Superior ore-masses seems also well sustained by the phenomena which they exhibit. They alternate with trappean ridges whose eruptive origin cannot be doubted, and which, themselves, contain so much magnetic oxide disseminated through their mass, as one of their essential ingredients, that they might almost be called ores. These eruptive masses include the largest and purest deposits of ore which are known in the Lake Superior or the Missouri iron regions; but there are other localities in both these districts where the mode of occurrence of the ore is somewhat different, and where the evidences of a direct igneous origin are less marked. This class comprehends those lenticular masses of ore which are usually included within gneissoidal rocks, and whose dip and strike coincide with that of the gneiss itself, but whose dimensions are limited. Such is the character of most of the Swedish deposits, and of many of those of Northern New York. Such beds of ore as these may in some cases be the result of segregating action; but the facts seem rather to indicate that they are made up of the ruins of pre-existing igneous masses, which have been broken and worn down during the turbulent action which we may suppose to have been pre-eminently manifested during the azoic epoch, and then swept away by currents, and deposited in the depressions of the sedimentary strata then in process of formation. In confirmation of this hypothesis in regard to the origin of these lenticular masses of ore in the gneissoidal rocks, it may be noticed that the ores occurring in this form and position are less pure than those of decidedly igneous origin, as if they had become more or less mixed with sand during the process of reconstruction, so that they not unfrequently require to be separated from their earthy impurities by washing before they can be advantageously used. Again, it may be observed in the case of some of the ore-beds of this class, that the bed-rock or foot-wall is considerably rougher or more irregular in its outline than the hanging wall or roof, as if depositions had taken place upon a surface originally rough and uneven, the upper surface of the ore being considerably smoother and more regular than the lower one, and sometimes separated from the rock above by a thin seam of calcareous matter.

There is still another form of deposit which is not unfrequently met with in the Lake Superior region, and which may be seen on a grand scale in the Pilot Knob of Missouri. This consists of a series of quartzose beds of great thickness, and passing gradually into specular iron, which frequently forms bands of nearly pure ore, alternating with bands of quartz more or less mixed with the same substance. Some of the deposits in the Lake Superior region are of this class, and they are very extensive, and capable of furnishing a vast amount of ore, although most of it is so mixed with silicious matter as to require separating by washing before use. Heavy beds of nearly pure ore occur at the Pilot Knob, interstratified with beds of a poorer quality. Deposits of this character are usually very distinctly bedded, and the ore shows a greater tendency to cleave into thin laminæ parallel with the bedding, in proportion to its freedom from silicious matter. These deposits seem to have been of sedimentary origin, having been originally strata of silicious sand, which has since been metamorphosed. The iron ore may have been introduced either by the sublimation of metalliferous vapors from below during the deposition of the silicious particles, or by precipitation from a ferriferous solution, in which the stratified rocks were in process of formation.

The great deposits of ore which have been alluded to above, all agreeing as they do in the characteristic features of their mode of occurrence, especially in the magnitude of the scale on which they are developed, are all, beyond a doubt, situated in the same geological position; they all belong to the oldest known system of rocks, the azoic. This name was first applied by Murchison to the ferriferous rocks of Scandinavia, and the geological position of the great iron regions of this country is precisely similar to those of Sweden. There is ample evidence that the lowest known fossiliferous strata, characterized by the same peculiar types of organic life, both in this country and in Europe, rest uniformly upon the iron-bearing strata throughout the Northwest, from New York to Missouri and Arkansas.

We have thus seen that the earliest geological epoch was characterized by the presence of the ores of iron in quantity far exceeding that of any succeeding one; indeed, we may infer that the ruins of the iron ores of this class have furnished the material from which many of the ores of more recent geological age may have been derived. The condition of things in reference to the ores of iron which existed during the azoic period underwent a complete change, and rarely do we find in any fossiliferous rocks any signs of unmistakably eruptive ores. It is certain that we nowhere out of the azoic system find masses of ore of such extent and purity as those which have just been alluded to. By far the larger portion of the azoic series on the earth's surface being covered up by the fossiliferous rocks, the ore which that formation contains is equally concealed, and it is only in those regions where no deposition of newer strata upon the oldest rocks has taken place that the treasures of iron are made accessible. In this respect, our country is pre-eminently favored, and there can be no doubt that the immense deposits of iron ore stowed away in the Northwest are destined at some future time to add to our national wealth more than has been or ever will be contributed by the gold of California. It may seem absurd to speculate on the exhaustion of the stratified ores of England or of the Eastern United States; yet nothing is more certain, than that the present rate of production in the former country cannot be kept up for any very great length of time, without making the cost of procuring ore so great, that other regions which now seem very remote from a market will be able to compete with the most favored iron-producing districts of England.

Practically, the views which have been presented above are of importance, as leading us to expect large and valuable deposits of the ores of iron wherever the azoic rocks are found to exist over any considerable surface. Thus it may safely be predicted that important discoveries of ore will be made in the now almost unexplored regions of British America, which are covered by rocks of the azoic period. Indeed, large beds of ore have already been found in Canada, which are, in character and position, analogous to those of Northern New York.

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3. On the Grooving and Polishing of Hard Rocks and Minerals by Dry Sand. By William P. Blake, of Washington.

The phenomena about to be described were observed in the Pass of San Bernardino (California), in 1853.* This pass is one of the principal breaks through the southern prolongation of the Sierra Nevada, and connects the Pacific slope with the broad and low interior plain of the Colorado Desert. It is bounded on each side by high mountains; the peak of San Bernardino rising on the north to the height of about 8,500 feet, and San Gorgonio on the south to about 7,000. The elevation of the summit-level is 2,808 feet above the Pacific, and the width of the gap at that point is about two miles; from this the ground slopes each way very gradually, the grade or descent on the east, for about 28 miles, being on an average 69 feet per mile.

On this eastern declivity of the pass, — the side turned towards the desert, — the granite and associate rocks which form the sharp peak of San Gorgonio extend down to the valley of the pass in a succession of sharp ridges, which, being devoid of soil and of vegetation, stand out in bold and rugged outlines against the clear, unclouded sky of that desert region.

* A brief notice of these phenomena is given in the writer's Preliminary Geological Report, accompanying the Report of Lieutenant R. S. Williamson of a reconnoissance in California. House Doc. 129, p. 27. Washington, 1855.

It was on these projecting spurs of San Gorgonio that the phenomena of grooving were seen. The whole surface of the granite, over broad spaces, was cut into long and perfectly parallel grooves and little furrows, and every portion of it was beautifully smoothed, and, though very uneven, had a fine polish. For a moment it was impossible to realize the cause of all this abrasion, performed in a manner so peculiar. The action of glaciers and drift was thought of in succession; but the appearance of the surface was so entirely different from that of rocks which have been acted on by these agents, that I could not regard them as the cause. While contemplating these curious effects, the solution of the problem was presented. The wind was blowing very hard, and carried with it numerous little grains of sand. When I stooped down and glanced over the surface of the rocks, I saw that they were enveloped in an atmosphere of moving sand, which was passing over and accumulating in deep banks and drifts on the lee side of the point. Grains of sand were thus pouring over the rocks in countless myriads, under the influence of the powerful current of air which seems to sweep constantly through this pass from the ocean to the interior.

Wherever I turned my eyes, — on the horizontal tables of rock, or on the vertical faces turned to the wind, — the effects of the sand were visible: there was not a point untouched, — the grains had engraved their track on every stone. Even quartz was cut away and polished; garnets and tourmaline were also cut, and left with polished surfaces. Masses of limestone looked as if they had been partly dissolved, and resembled specimens of rock-salt that had been allowed to deliquesce in moist air. These minerals were unequally abraded, and in the order of their hardness; the wear upon the feldspar of the granite being the most rapid, and the garnets being affected least. Wherever a garnet or a lump of quartz was imbedded in compact feldspar, and favorably presented to the action of the sand, the feldspar was cut away *around* the hard mineral, which was thus left standing in relief above the general surface. A portion, however, of the feldspar on the lee side of the garnets, being protected from the action of the sand by the superior hardness of the gem, also stood out in relief, forming an elevated string, osar-like, under their lee.

When the surface acted on was vertical, and charged with garnets, a very peculiar result was produced; the garnets were left standing in relief, mounted on the end of a long pedicle of feldspar, which had been protected from action while the surrounding parts were cut away. These little needles of feldspar, tipped with garnets, stood out from the body of the rock in horizontal lines, pointing, like jewelled fingers, in the direction of the prevailing wind. They form, in reality, a perfect index of the wind's direction, recording it with as much accuracy as the oak-trees do in the regions about San Francisco, where they are all bent from the perpendicular in one direction, or in some places lie trailed along the ground. All these little fingers of stone pointed westward, in the direction of the valley of the pass, to which the wind conforms. We experienced this wind before reaching the point of rocks and the sand-drifts; it blew with great force, and seemed to be a great air-current, as uniform in its direction and action as the great currents of the sea. It flows into the interior with singular persistence and velocity, sweeping down over the slope of the pass, not in fitful gusts and eddying whirls, but with a constant uniformity of motion, unlike any of the winds of our Atlantic seaboard, or of the plains.

The pass would in fact appear to be a great draught-channel or chimney to the interior, through which the air rushes inland from the cool sea, to supply the vacuum caused by the ascent of a column of heated air from the parched surface of the great Desert. This pass is the only break of any magnitude in the mountain chain for a long distance, and, as an air-channel, holds the same relation to the Colorado Desert as is sustained by the Golden Gate at San Francisco to the broad interior valleys of the Sacramento and San Joaquin.

The effects of driving sand are not confined to the pass; they may be seen on all parts of the Desert, where there are any hard rocks or minerals to be acted upon. On the upper plain, north of the Sand Hills, where steady and high winds prevail, and the surface is paved with pebbles of various colors, the latter are all polished to such a degree that they glisten in the sun's rays, and seem to be formed by art. The polish is not like that produced by the lapidary, but looks more like lacquered ware, or as if the pebbles had been oiled or varnished.

On the lower parts of the Desert, or wherever there is a specimen of silicified wood, the sand has registered its action. It seems to have been ceaselessly at work, and where no obstacle was encountered on which wear and abrasion could be effected, the grains have acted on each other, and by constantly coming in contact have worn away all their little asperities, and become almost perfect spheres. This form is evident when the sand is examined by a microscope.

We may regard these results as most interesting examples of the denuding power of loose materials transported by currents in a fluid. If we can have a distinct abrasion and rectilinear grooving of the hardest rocks and minerals, by the mere action of little grains of sand falling in constant succession, and bounding along on their surface, what may we not expect from the action of pebbles and boulders of great size and weight transported by a constant current in the more dense fluid, water? We may conclude that long rectilinear furrows, of indefinite depth, may be made by loose materials, and that it is not essential to their formation that the rocks and gravel, acting as chisels or gravers, should be pressed down by violence, or imbedded in ice, or moved forward *en masse* under pressure by the action of glaciers or stranded icebergs. Wherever, therefore, we find on the surfaces of mountains, not covered by glaciers, grooved and polished surfaces, with the furrows extending in long parallel lines, seeming to indicate the action of a former glacier, we should remember the effects which may be produced during a long period of time by light and loose materials transported in a current of air; and which consequently may be produced with greater distinctness, and in a different style, by rocks moved forward in a current of water. The effects produced by glaciers, by drift, or moving sand, are doubtless different and peculiar, — so different and characteristic, that the cause may be at once assigned by the experienced observer, who can distinguish between them without difficulty. It is, however, possible, that, after a sand-worn surface, such as has been described, has been for ages covered with moist earth, a decomposition of the surface would take place, sufficient to remove the polish from the furrows, and leave us in doubt as to their origin.

If it were possible, it would be exceedingly interesting to ascertain the length of time it has required for the little grains of sand to carve the surface of the granite ridge to its present form. How inappreciably small must be the effect produced by a single grain! And yet by their combined and long-continued action mighty effects are produced. That the action of the grains singly is not visible, is proved to us by the polished surface, for no one grain cuts deeply enough to leave a scratch.

Ages have, doubtless, elapsed since this action of the sand began, and we cannot tell how deep the abrasion has extended; cubic yards of granite may have been cut into dust, and driven before the wind over the expanse of the Desert.

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4. Observations on the Characters and Probable Geological Age of the Sandstone Formation of San Francisco. By William P. Blake, of Washington.

(*Abstract.*)

The hills upon which the city of San Francisco is built are formed of regularly stratified sandstone, in thick beds, alternating with shales. This sandstone has many interesting peculiarities, being very hard, and of a dark bluish-green color in the interior portions; when exposed to the air and moisture, it rapidly changes, becomes rusted and brown, and is much softened. It is therefore not very suitable for building, and yet in dry situations will form a very durable wall.

These strata crop out in different parts of the city, and have been exposed by street cuttings; they have also been exposed by several quarries on the islands near the city and around the bay. These openings show that the strata are much uplifted; they dip at all angles, from 20 to 60 degrees, and appear to be thrown into great wave-like flexures. Several small faults and dislocations of the beds were seen in different places, and show that the formation has been greatly disturbed.

This sandstone is compact, fine-grained, and hard; it is not easily crumbled, and is quite tough. After long exposure, however, it may be crumbled by the fingers on the edges of the blocks. All the specimens that I examined contained a notable quantity of carbonate of lime. Iron pyrites was also observed. The sandstone and the shales are filled with numerous thin black films, or small patches, which appear to be of vegetable origin. In the sandstone they have the characters of a hard and dark slaty clay; in the shale, however, masses of similar form are distinctly carbonaceous.

I have not been able to obtain any fossils in the quarries about San Francisco, or to find any in the outcrops of the strata. At Benicia, however, there are similar sandstones; and I there found casts of *Turritella*, and a shark's tooth which Professor Agassiz has recognized as of the genus *Lamna*. These strata are much softer than the beds at San Francisco, but they are also much uplifted, and some hard, undecomposed blocks were seen, which greatly resemble the sandstone of Yerba Buena Island, opposite the city. I am therefore of the opinion that the Benicia strata belong to the formation developed around San Francisco. South of Benicia, around the base of Monte Diablo, similar strata are abundantly developed; and at one point I found imperfect casts of a species of *Cardium*. I have obtained also a block of sandstone from the coal formation of Bellingham Bay, which in its mineral characters resembles very closely the San Francisco rock. It contains fragments of coal, and impressions of coal plants, and has two large, well-preserved shells of the genus *Pecten* imbedded in it.

With these facts in view, I have been induced to regard the San Francisco sandstone as of *Tertiary age*, although I have found it difficult to so regard it by reason of the peculiar hardness and compact character of the strata, which more closely resemble the older palæozoic formations than ordinary Tertiary.

I have recently procured additional evidence of the age of these rocks from several fossil *Spatangi* which were washed up by the surges of the Pacific on the beach of the western side of the San Francisco peninsula. I present it here, with the hope that Professor Agassiz may be able to assign its age.

These Spatangi are thrown up in considerable quantities, though it is difficult to obtain perfect specimens. The color and mineral characters of the investing rock are so nearly identical with those of the sandstone around the city, that I do not hesitate to regard the fossils as broken from submarine outcrops of the strata.

Between San Francisco and the Pacific there is a high ridge of serpentinoid or magnesian rock, which is flanked on both sides by the sandstone, and which appears to be eruptive. A mass of sandstone and shale, about 300 feet thick, is imbedded in this serpentine.

A belt of hard, flinty, or jasper rocks crops out near the Presidio; they are distinctly stratiform, and dip at a high angle. They are charged with irregular veins of white quartz. These rocks are probably metamorphosed portions of the sandstone formation. Similar rocks are largely developed at Lime Point and at San José. At the

San Juan Mountain, on the road to Monterey, there are also heavy strata of sandstone, highly inclined, and resting on granite; which is also the case at San José.

It would therefore appear that the granitic axis of the San Francisco range of the Coast Mountains *is newer than this sandstone.* The same is probably true of the other ranges. If, therefore, this sandstone formation proves to be Tertiary, as I am forced to believe from all the evidence I can obtain, we must assign a post-Tertiary age to the Coast Mountains, at least in the region of San Francisco.

The connection and apparent synchronism indicated between the rocks of the coal formation of Puget Sound and those of San Francisco, both by the similarity of mineral character and the universal presence of patches of lignite, is highly interesting, and worthy of note; leading, as it does, to the conclusion that valuable beds of Tertiary coal may yet be found in the Coast Mountains near San Francisco, or where the formation is developed.

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5. Remarks upon the Geology of California, from Observations in Connection with the United States Surveys and Explorations for a Railroad Route to the Pacific. By William P. Blake, of Washington.

(*Abstract.*)

Before the surveys of Lieutenant Williamson, in 1853, the topography and direction of the mountains between Walker's Pass and the Gulf of California were but little known. The surveys have shown that the lower portion of the Sierra Nevada, south of Walker's Pass, curves southwesterly, and unites with the Coast Mountains af the head of the Tulare Valley. The ridges in that latitude (35°) are not snowy, their elevation being less than 7,000 feet. South of this point of junction of the Sierra Nevada and the Coast Mountains a chain of high ridges is prolonged, and trends nearly east and west. It extends eastward from the Sierra Nevada to Mount San Bernardino, and forms the southern boundary of the Great Basin. This chain appears to be prolonged in the same east and west direction to the coast at Point Conception and Arguila, being in fact a *transverse chain* nearly at right angles with the direction of the Coast Mountains and the Sierra Nevada. This chain extends from Point Conception to San Bernardino, and I have proposed for it the appellation *Bernardino Sierra.*

At San Bernardino the trend of the mountains again changes, and becomes more nearly north and south. At the base of the peak of San Bernardino is the wide and low pass of the same name (also called San Gorgonio), which, so far as is known, is the best pass in the whole chain south of the Columbia River. From this pass southwardly, there is a continuous chain of high ridges to the end of the peninsula at Cape St. Lucas. For this chain I have proposed the name of *Peninsula Sierra.* It forms in its northern part the water-shed and barrier between the Pacific and the Colorado Desert, and between San Diego and the mouth of the Gila River. It has an average elevation in its northern part of about 6,000 feet.

The rocks of all these chains are chiefly granite, gneiss, and mica slate, including beds of white limestone and quartz rock. It was generally found that the central or higher parts of the Sierra Nevada were of compact granite; but even this was not free from a structural arrangement of the minerals. At the Tejon Pass, the rock was found to be filled with lenticular beds of hornblende, or hornblende and mica, trending in the same direction with the more highly laminated portions, or the mica slate. On the eastern slope of this pass, thick beds of white limestone, containing graphite, were observed, together with quartz rock, which are considered as indicating a sedimentary origin.

In the Bernardino Sierra, the granitic series is somewhat different; the rock is whiter, and contains a greater amount of feldspar or albite. The slaty or laminated form of granite is however found, and talcose slates bearing gold were seen. Several intruded ridges of porphyry were also found in this range. The eastern side of the northern part of the Peninsula Sierra consists chiefly of gneiss and mica slate; the rock of the western side is more compact, and is covered with soil and verdure, while the other side is bare and barren.

None of the palæozoic or older stratified rocks were seen during the survey; — they are either absent or have been metamorphosed. The only stratified formations are those of the Tertiary age, and the more recent deposits. The Tertiary strata flank the granitic elevations described, and rest horizontally upon the upturned edges of the slates.

The principal point where Tertiary strata are developed and characterized by fossils, is at Posé Creek, near the Tejon Pass. They consist chiefly of volcanic sands and pumice-stone, with argillaceous and sandy beds. The latter contains numerous fossils, among which species of the genera *Pecten*, *Natica*, and *Meretrix* are abundant. They are considered by Mr. Conrad as of the Miocene division of the Tertiary. Numerous sharks' teeth were also obtained from this formation, at an elevation of near 1,700 feet above the sea. Tertiary fossils were also obtained at the Cañada de las Uvas, San Fernando, San Diego, and at the margin of the Colorado Desert, along the banks of Carrizo Creek. Those obtained at the Cañada de las Uvas are from the Eocene division of the Tertiary, being well characterized by the well-known fossil shell *Cardita planicosta*.

The alluvial formations of California cover a broad area. The Sacramento and San Joaquin rivers form extended interior deltas, and the Tulare lakes are bordered by wide plains of barren clay, evidently of lacustrine origin. The immediate borders of the lakes are covered with a thick growth of gigantic bulrushes (*tulé*), and the shores being shelving, and the lakes shallow, they become much expanded during the rainy season, when the streams from the mountains are much swollen. King's River and the Ca-wee-ya are large streams, and form broad deltas covered with timber where they enter the lakes. The great valley or low plain of the Colorado Desert, lying far to the south of the Tulare Valley, and extending northwards from the head of the Gulf of California, is also of alluvial or lacustrine origin, and consists of a deep blue clay, which forms a hard, level surface. This clay is charged with small fresh-water shells, and they are found abundantly on the surface. This clay was proved to be of lacustrine origin by the existence of a distinct beach-line at the base of the hills, marking the former level of a sheet of water. This beach-line was distinctly developed, and could be seen on the other side of the valley, ten or fifteen miles distant. On the high ridges that extended out from the main chain into the valley a distinct water-line was found, and a calcareous incrustation two feet thick on the rocks, it having been deposited by the water. The evidences of the former existence of a vast interior lake were here seen on all sides, and in some places accumulations of boulders and pebbles were found strewed upon the surface, and so incrusted that it was evident they had not been disturbed since they were covered by the water.

I offer the following as the best explanation of the formation and subsequent desiccation of this lake. The Gulf of California probably extended to the head of the valley, 170 miles north of its present limits. The silt of the Colorado was probably deposited across this former extension of the Gulf, so as finally to shut off the upper portion, and leave it as a lake. The water has probably been evaporated by the dry winds of that desert region. There is reason to consider that the bottom of this ancient lake, now dry, is below the level of the Gulf; a great depression being indicated by the barometrical observations, and shown more conclusively by the fact that the waters of the Colorado, at times of freshets, overflow and run back into the Desert for many miles.

Further observations on the alluvial formations and the sands of the Desert were given, and remarks made upon the rocks of the coast near San Francisco.

The remarks of Mr. Blake were illustrated by a colored map, and at the conclusion a series of observations and remarks were made by Professors Agassiz, Guyot, Hall, and Dana, and by Dr. Le Conte and Mr. Leslie.

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6. Description of several Sections measured across the Sandstone and Trap of Connecticut River Valley in Massachusetts. By Professor Edward Hitchcock, of Amherst.

Three sections were exhibited: one crossing in the latitude of Turner's Falls; another in that of Mount Tom; and a third in that of Chicopee. A geological map of the line of the section, with the strike of the strata and the localities of footmarks, accompanied each. The strike and dip, as well as the distances, were obtained by the usual instruments; the heights by the aneroid barometer. Other sections are yet to be measured; but those exhibited lead to some important conclusions.

1. The hypozoic rocks, which terminate two of the sections, (the ends of the other being granite,) have a large dip towards the axis of the valley.

2. The trap ranges, crossed somewhat west of the middle of the

sections, form beds between the sandstone strata, and seem to have produced but little disturbance in the sandstone.

3. The strike of the sandstone varies from N. and S. to N. E. and S. W., — in some places even to E. and W.; but the usual strike is N. from 20° to 30° E. The dip is almost universally E. and S. E., from 5° to 50°.

4. The sandstone, lithologically considered, exhibits three distinct varieties: — 1. The rock lying west of or below the trap ranges is almost universally a thick-bedded coarse sandstone, passing into a conglomerate; the pebbles being granite, mica slate, and quartz. In the vicinity of the trap the color is often gray, and sometimes white. 2. A group of micaceous sandstones and shales of various colors, with thicker beds of deep-red and rather fine-grained sandstone. These lie immediately upon the trap, or to the east of it. 3. A very coarse gray conglomerate, the fragments often several feet in diameter, consisting of granite, gneiss, and micaceous and argillaceous slates. This deposit, which has been swept away, except a few isolated hills, lies the highest in the series, and towards its lower part is interstratified with the shales and micaceous sandstones, as in Mount Mettawampe.

5. With a single exception, the fossil footmarks occur in the second of the above groups of the formation, and chiefly near its lower part, where it rests on the trap. In a single case only they have been found immediately beneath the trap (on Holyoke). The fossil fishes all occur just above the trap; but the *Clathropteris* immediately below it (on Tom). With these two exceptions, nothing organic but fucoids has been found in the lower group, — the thick-bedded sandstone, — unless it be some fragments of dicotyledonous trees.

6. The sandstone has been raised since its deposition; for in some places (Turner's Falls), the most delicate footmarks occur on strata having a dip of over 40°; and the idea of animals walking on mud lying at such an inclination is absurd.

7. The perpendicular thickness of the sandstone on the different sections is as follows, estimated trigonometrically from the measurements in the usual way: —

	Feet.
1st section across Turner's Falls, ...	14,124
2d do. across Mount Tom, ...	12,241
3d do through Chicopee, ...	13,596
Mean, ...	13,320

Suppose we allow one half of this for original deposition, we have still left a thickness of over 6,000 feet. If there are faults, I have no certain evidence of their existence.

8. The discovery of the Clathropteris by my son, Edward Hitchcock, Jr., M. D., beneath the trap of Mount Tom, makes it very probable that the zone of rock in which it occurs belongs either to the upper part of the Trias, or the lower part of the Lias, where alone this fern is found in Europe. This would make all the rocks above the trap newer than the Trias, probably. Below this zone we have thickness abundant, according to my measurements, not only for the Trias, but also for the Permian, and perhaps still lower formations. And it is my opinion that the three groups of strata above pointed out are distinct enough in character and organic remains, to be regarded as separate formations. The two groups above the trap are thick enough, not only for the Jurassic series, but for the Cretaceous also.

9. The trap rocks were not the agency by which the sandstone of this valley was tilted up. For the dip is as great, and usually greater, beneath than above the trap. This latter rock was probably an overflow upon the bed of the ocean in which the sandstone was deposited; as the volcanic grit, lying above the compact trap, certainly was.

10. The agency by which this sandstone was raised, was probably the falcating lateral force that ridged up the Apallachian range of mountains generally; since the strike has a general correspondence with the direction of that chain. That force may have been exerted at several periods, and this may have been one of the last. The high dip of the hypozoic rocks towards the axis of the valley, as shown on the sections, indicates an approach of the sides of the valley, which must have crowded up the layers of sandstone.

The author of this paper professed an intention to measure other sections across this valley in Massachusetts, and expressed a hope that the same might be done in Connecticut; which, if done, may modify some of the preceding conclusions.

7. ADDITIONAL FACTS RESPECTING THE TRACKS OF THE OTOZOUM MOODII ON THE LIASSIC SANDSTONE OF THE CONNECTICUT VALLEY. By PROFESSOR EDWARD HITCHCOCK, of Amherst.

SOME slight resemblance between the phalangeal impressions of this animal and the foot of an embryo frog led the author of this paper some years ago to refer it to the Batrachian family. He presented two other facts to strengthen this analogy.

1. The foot was possessed of a web, extending even beyond the toes. The specimens show this distinctly, especially some recently discovered; and it must have buoyed up the animal on the mud, like huge snow-shoes. Yet it sunk quite deep, and was, therefore, very heavy.

2. The toes were not terminated by claws, but by knobs. In this respect they correspond to many species of Batrachians.

At least two new localities of this extraordinary track have been lately discovered: one at Turner's Falls in Gill, and the other at the quarries in Portland, Ct.

The Otozoum was doubtless one of the most extraordinary animals that ever trod the earth. With a foot twenty inches long, and covering more than a square foot of surface, it must have been of gigantic size; and though apparently of Batrachian type, a biped Batrachian of such dimensions must have had great peculiarities of structure. The new *Ichnological Museum* of Amherst College, erected by the liberality of the trustees of the late Samuel Appleton, will contain numerous specimens of these tracks, for the study of future naturalists.

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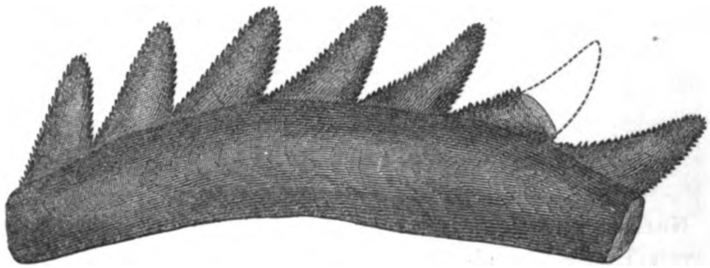
8. TRACES OF ANCIENT GLACIERS IN NEW ENGLAND. By PROFESSOR EDWARD HITCHCOCK.

9. INFERENCES FROM FACTS RESPECTING THE EROSIONS OF THE EARTH'S SURFACE, ESPECIALLY BY RIVERS. By PROFESSOR EDWARD HITCHCOCK.

As these two papers require numerous drawings, to be made intelligible, and will probably erelong be published in another form, the author has judged it inexpedient to give even an outline of their contents in this place.

10. Account of the Discovery of the Fossil Jaw of an Extinct Family of Sharks, from the Coal Formation. By Professor Edward Hitchcock, of Amherst.



This beautiful and remarkable specimen was sent to the author of this paper by Rev. John Hawks, of Montezuma, in Indiana. It was given to him, to be sent to Professor Hitchcock, by Dr. S. B. Bushnell, of the same place. Mr. Hawks says: "It was found in Park County, Indiana, near the Wabash River, in a layer of slate, about one foot beneath the surface of the ground. Immediately beneath the layer of slate, which was about one foot in thickness, was a *coal bank.*"

Dr. Hitchcock stated that this specimen was evidently the jaw of a shark, but of very peculiar character. But it would be presumption in him to venture to throw out any suggestions concerning it, when we had one present to whom all the wonders of ichthyology, both recent and fossil, were as household words. He therefore called on Professor Agassiz to give his views respecting the specimen.

Professor Agassiz gave it as his decided opinion, that this specimen formed a part of the jaw of a shark, allied to the saw-fish, or Pristis family. He stated that the sword of the Pristis is originally composed of two bones, and if these should continue separated, each part, with teeth on only one side, would not be much unlike the fossil specimen. This is curved, however, and the teeth have fine serratures. He supposed that the fish had a corresponding jaw projecting from the opposite side of its head, and that both formed a powerful weapon of offence. He regarded it not only as an undescribed genus, but belonging to a new family of fishes; and spoke of the discovery as of great importance in fossil ichthyology; scarcely inferior to that of the Ichthyosaurus and Plesiosaurus in fossil herpetology.

The author of the paper will commit the specimen to Professor Agassiz to be described. An accurate outline is appended to this historical sketch. (See figure.)

IV. BOTANY.

1. Note upon a Peculiarity of the "Redwood" (Genus Sequoia). By William P. Blake, of Washington.

The redwood, which is now used so largely for construction in the city of San Francisco, has the peculiarity of turning black, like ebony, when treated with an alkali. If a panel of this wood be prepared with a smooth surface, and then washed with a moderately dilute solution of caustic potash, the light red color — like that of our ordinary pencil cedar — is immediately changed to black, and the grain of the wood is beautifully developed. The carbonates of potash and soda produce the same result, but not so rapidly; it may also be accomplished by lime-water or ammonia. The effect is produced with most ease when the wood is freshly cut or sawn, and the pores are filled with the undried juices. A decoction of the wood is turned black in a similar manner; and I suggest that it may be used advantageously in chemical analysis as a test for alkalies. It would probably make very good test-paper, and the reaction would be more evident and striking than with turmeric. When the alkaline solution is weak, the color produced on the wood is not a deep black, but has a shade of red, and resembles old dark mahogany, or rosewood. From the ease and cheapness with which the effect may be produced on the wood, and the extreme beauty of the panels thus prepared, it will doubtless become common to stain cabinet furniture and doors in this manner, in preference to painting.

2. Notes upon the Grove of "Mammoth Trees" in Calaveras County, California. By William P. Blake, of Washington.

The grove of the mammoth trees is near the sources of the Calaveras River, in a depression between the hills, about 15 miles from the mining village called Murphy's. This little valley is said to be but 15 miles from the snow-line of the high ridges of the Sierra, and 30 to 35 miles from the crest. Its elevation above the sea has been approximately determined by the engineer of the Union Water Company, who considers it as 4,550 feet, and as 2,400 feet above Murphy's Camp.

The valley is densely wooded with evergreen trees, principally pines and spruces, and in some portions there is a thick undergrowth, which covers the ground from view. The great trees tower above this forest, — itself gigantic, — and present a magnificent spectacle when viewed from the surrounding hills. Mr. Lapham, who has erected a hotel in the grove, states that he has counted about 190 trees, including young and old. I saw about 20 of large size, and several young ones. They are irregularly distributed over a space a quarter of a mile square, and are generally in groups of two or three together. The principal trees have received fanciful names, such as *Father of the Forest*, *Beauty of the Forest*, *Three Sisters*, *Mammoth*, &c. Their size is variable, the diameters ranging from 15 feet to over 30, and the heights from 250 to 360 feet. The stump of the only tree that has been cut has been smoothed down, and made perfectly level, and now forms the floor of a ball-room. The solid wood of this stump, exclusive of the bark, measures 25 feet in diameter, and, with the bark, is about 30 feet. This is about 4 feet from the ground, where its diameter is probably 32 feet. The largest tree now standing, called the Mammoth, is of about the same size, but is imperfect on one side, a portion having been burnt out. I measured this tree carefully with a tape, and found it 94 feet in circumference at the roots, giving a diameter of over 31 feet. Although I did not measure the heights of these trees, I saw no reason to question the accuracy of the statements that have been made regarding them, and I believe them to range from 300 to 360 feet. One is said to be 363 feet, and an old one, lying prostrate, and much decayed, appears to have been over 400 feet high. This prostrate tree is hollow, and I walked through it erect for a long distance; and it is said that, before the lower part became filled up by earth and stones, brought in by a brook, a man could ride through on horseback.

The tree which was cut down was partly used for lumber, and a part of the top has been levelled off, and a full-length double bowling-alley built on it. The log tapers so gradually, and its great diameter is so well preserved for a long distance, that at the extreme end of the alley it is not possible to get from the ground to the top of the log without climbing up by the limbs. This tree could not be felled by the ordinary method, and boring was resorted to, a long pump-augur being used for the purpose. The bark was stripped off from the lower part of the log, and sent away for exhibition. Since that time a second tree has been stripped of its bark to the height of 90 feet, and the tree is still standing. It is the intention of the proprietors of the grove to surround it with a spiral staircase, so that visitors may ascend to the top, and have a full view of the surrounding forest, with here and there a giant trunk rising above it.

The geology of the valley appears to be very simple. A hard, compact, gray granite was seen in the vicinity of the trees; it appears to be the underlying rock, and the soil is composed of its *débris*. A well has been dug at the house of Mr. Lapham, and the material removed shows that the soil is deep, and contains considerable fine white clay. Some rounded blocks of basaltic or trappean rocks are found on the surrounding hills.

The temperature of the valley is said to be mild, even in winter, the ground rarely freezing; but the snow generally lies 30 inches deep from January 1st until April. At the time of my visit (the first week in August, 1854) strawberries were just ripening, and columbines were in bloom. The heat of the sun's rays was very great at midday, but the shade of the forest was cool and delightful. The nights were also very cool.

The valley is abundantly watered; in the spring it is quite wet, and a good-sized brook flows through it.

3. LETTER ADDRESSED TO DR. JOHN TORREY, ON THE AMMOBROMA SONORÆ. By MR. A. B. GRAY. (Communicated to the Association by DR. TORREY.)

New York, October 20th, 1854.

DEAR SIR: — It affords me much pleasure to comply with your request, respecting the particulars of the plant now in your possession, which I discovered near the head of the Gulf of California, on my recent reconnoissance through the Mexican State of Sonora.

I had reached an Indian rancho, called "Sonoita," situated about fifty miles from the Gulf coast, and being desirous of extending my barometric observations to the level of the sea, as also to fix the latitude and note the character of the country in the vicinity of (what is called) "Adair Bay," it became necessary to obtain a guide. After much consideration on his part, and many Indian presents given him, I persuaded the "Governador," or head chief, of the Papijos of that region, to agree to conduct me to the sea-shore, and to this bay. On the third day of my travel (Wednesday, 17th May, 1854), under the guidance of the old chief, and after passing over an immense bed of basalt and volcanic lava, we came to a range of numberless sand-hills, extending southeasterly and northwesterly. These hills were of loose shifting sand, of about thirty feet summit-elevation above the surrounding plain, and about five miles broad. In a southwest direction they seemed to terminate at six miles' distance, but to the northwest they appeared to extend as far as could be seen with the telescope.

Immediately upon entering these sand-hills, our course being across them in a westerly direction, I observed the Indian dismount from his horse, and commence digging with his hands. At first I could not perceive his object, but shortly discovered that he had pulled out of the sand a vegetable-looking substance, which was shaped somewhat like a mushroom. He showed great eagerness to obtain more, and made a sign that it was good to eat.

Upon examination, I found this plant had a sort of pestle-shape, with a stem extending perpendicularly below the surface for several feet, and a round top of about two inches in diameter, partly protruding above the sand, say an eighth of an inch, the body or stem of the plant about three quarters of an inch in diameter near the top, and two feet below somewhat larger; this part was not so brittle as the stalk of the mushroom, but more of the tenacity of asparagus, with a light orange-colored tinge on the outside, and the inside of a dull whitish color.

I noticed in pulling it up that it would snap off, and in but few instances did we secure any of its roots. It appeared to be either attached to something else, or to have a deep underground growth. The longest that I saw was about three and a half feet, but this was not the entire length, as it was broken off in pulling up. The top or head part was of a spongy substance, with small blue and purple pistils. The Indian gathered a bundle of them, breaking off and casting away the top of each. We encamped for the night in the sand-hills, and the chief, instead of supping with us, as usual, made a fire, and roasted his roots or plants on the hot coals (which took about twenty minutes), and commenced eating them. None of the party seemed inclined to taste, but, out of curiosity, I moved over to the chief's fire, and he handed me one. At first I ate but little, and slowly, but in a few minutes, so luscious was it that I forgot my own mess, and ate heartily of it; next morning each of the party "followed suit," and afterwards there was scarcely enough gathered to satisfy us. The taste, though peculiar, was not unlike the sweet potatoe, but more delicate. When first taken from the ground, and before cooking, it tasted more like the raw ground artichoke. The day following we noticed wigwams, and traces of Indians, which the chief made a sign to us were old encampments of tribes who lived at the Gulf shore, and who had been to the sand-hills to gather this vegetable. At noon we came upon a body of Indians encamped upon a ridge of sand, who, upon seeing us approach, made every preparation for a hostile meeting. Our chief, however, after a couple of hours' talk with them, induced kindlier feelings, when they brought back their women and children, who had been sent to a distance on our first appearance. We shortly after encamped, when they brought us dried fish, and a sort of *pinole*. The Mexican pinole is made of parched wheat, or corn, ground upon a *metat* (or stone), but of an entirely different color and taste from that of these Indians. They made signs that it was the dried vegetable ground into meal, and mixed with wheat. I ate of this pinole, and found it very nutricious.

The plant, when dried, or allowed to remain in the air a few days, becomes hard and dark, with an odor approaching to the Vanilla bean. In this state it becomes one of the principal articles of food with the tribes we met at the Gulf, they chewing it, as well as grinding it for pinole. They also get fish and crabs, which we found in abundance on the coast. They have nets, and I observed they had a great many *hawks* domesticated, but could not learn what use they made of them. The growth of vegetation in the vicinity of the sand-hills was chiefly stinted mesquit, cacti, iron-wood (called so by the Mexicans), and salt and hard cane grasses. The "iron-wood" bore a purple-colored pea-shaped blossom, was in full bloom at the time, a thorny shrub, and without leaves.

My party had just accomplished a journey and exploration of 1,700 miles, a great part of the way without roads or trails, and had undergone many privations and hardships, without guides, other than the instruments I carried along, and the knowledge I procured of a portion of the route from a previous reconnoissance and survey from El Paso to San Diego in California. They were on very short rations, and our animals from hard service weak and giving out, and with two long and dreary deserts to cross before reaching the settlements. Still, after approaching so near the point I had desired to reach when I set out, I was determined to hazard much to accomplish it. Everything had to be done by signs, not being able among the Indians to find one that spoke Spanish, and we, of course, not understanding the Cocopa or Papego languages. On returning from the head of the Gulf, we ate of this vegetable for about four days in its fresh and dried state, and found no bad effects from it. The Indians made signs that we were the first white people that had ever been there. We noticed this peculiarity among them, that, although a lazy set of Indians, they were well made, and fat, but had very bad teeth, even from the youngest to the oldest.

The latitude of these sand-hills is 31° 45′, longitude 113° 40′, and about six miles from the Gulf shore. No rain had fallen there for six months, it being the dry season. I regret I could not procure many more particulars relative to this plant, which interested me much from the circumstances attending the finding it; but should you desire any further information that I possess, which might aid you in discovering what it is, I shall be happy to communicate it. Similar sand-hills I noticed a hundred miles off, near the junction of the Gila and Colorado, but none of the plant was found in them.

With great respect, I remain very truly yours,

A. B. GRAY.

NOTE.—This interesting plant will be described in full, and a figure of it given, in Mr. Gray's forthcoming Report of his explorations. It is evidently a new genus, nearly allied to the little known and anomalous Corallophyllum of Kunth, and the Pholisma of Nuttall. In the floral structure of scales it is more like the latter, from which it is distinguished by its woolly-plumose calyx, and its singular cyathiform inflorescence. The three genera may well be grouped into a small family, under the name of Corallophyllaceæ, or rather Lennoaceæ, as Lennoa is the older name of the typical genus. Lindley referred these plants to Monotropaceæ, in which he was nearer right than Nuttall, who placed Pholisma in Scrophulariaceæ.

JOHN TORREY.

V. PHYSIOLOGY.

1. LAWS OF REPRODUCTION, CONSIDERED WITH PARTICULAR REFERENCE TO THE INTERMARRIAGE OF FIRST-COUSINS. By REV. CHARLES BROOKS, of Medford, Mass.

WHETHER the intermarriage of near blood relations is accordant with, or opposed to, the laws of nature, is a question which should be confined primarily to physiological science. Believing it to be important that scientific men should examine it, I take the liberty of inviting to it the attention of the Association, any of whose naturalists are better able than I am to discuss it; and some of whom could so examine and so decide as to put the question at rest. The improvement and prosperity of thousands of families depend on its solution; and, in a degree, the safety and elevation of society.

It is my purpose only to indicate *the course of inquiry*, and not to draw conclusions. When the facts shall have been collected and arranged, then the fixed and benignant laws which underlie the great agencies of reproduction will be evolved. It is only by wide and thorough surveys that any reliable statements can be reached.

In examining the children of first-cousins, the temperaments of the parents must be specially considered; for it is thought that, where the temperaments are alike, the consequences are sure to be peculiarly disastrous; but where they are directly opposite, such consequences will not so appear. These results so much depend on temperaments, that they should always be classified accordingly.

Another circumstance to be regarded in these investigations is, the antecedents of the parents. Have any of their ancestors married first-cousins? and, if so, when did such marriages exist, and what were the health, pursuits, and character of the parties?

Other circumstances should be noted; such as climate, situation, and food. The effects may vary somewhat in high and low latitudes, in eminently civilized and eminently barbarous communities; and also where the marrying parties have indulged, through their youth, in great varieties of food; or where, as on small islands, and in the deep forests, they have been restricted to a fixed routine of meagre dishes.

Taking these and other related circumstances into view, we may begin to classify the facts thus:—

First. Those children, born of first-cousins, who are well developed in body, and vigorous in mind.

Second. Those feeble or peculiar in body only.

Third. Those feeble or peculiar in mind only.

Fourth. Those feeble or peculiar in body and mind.

The only way of settling the question is to take the census of a State. Take, for example, all the parents in Massachusetts who have married first-cousins; if a larger number of their children are found to die early, or to want a full development of body or mind, than the children of the same number of parents who have not married first-cousins, this will show that first-cousins should not marry together. Take one thousand couples who have married first-cousins; if more of the children of these parents are found to be imbecile, or defective in body or mind, than the children of *any* one thousand couples in the State found in similar conditions of life, this will prove that the marriage of such cousins is unwise. Again, if out of one thousand couples who have married first-cousins, there be particular families who have two, three, or even four children that seem weak or strange, then this fact would be a strong presumption against the expediency of marriage between such near blood relations. Nothing but a true census can furnish adequate elements for a just calculation. Suppose one hundred thousand married couples in Massachusetts, and that five thousand of them have married first-cousins. Then suppose that there are six thousand children, among all who have been born from them, who appear different from the rest. They are either weakly in body, or peculiar in mind, and seem strange to their neighbors; some perhaps were born blind, some deaf; some with too many fingers and toes, and some with too few; some without hair, and some with too much; some talking in a strange way, and dressing in a stranger; some abhorring society, and some abhorring solitude. Suppose these six thousand persons are acknowledged to be different from the rest of mankind. Now, if more of them are children of first-cousins than the proportion which five thousand bear to one hundred thousand, then it is plain that the marriage of first-cousins is productive of disastrous consequences. To state the same fact in a shorter form: if one hundred thousand couples have six thousand imbecile or peculiar children, then five thousand couples should have three hundred of them, as their proportion. If by an *adequate* census it shall appear that the five thousand couples who married first-cousins have from three hundred and fifty to four hundred imbecile or peculiar children, then it will be proved that the marriage of first-cousins is forbidden of God.

It would be a lasting glory to any scientific philanthropist who should collect and arrange all the facts existing in any State of this Union. The field of inquiry is narrowed by certain facts. The Roman Catholic Church, by positive statute, forbids the intermarriage of first-cousins. England condemns it; and the people there have some strong and harsh sayings concerning it. The Quakers forbid such marriages; and some Cantons of Switzerland have forbidden them by law. Tradition, in this country, brings down to us two or three sayings on this subject, which most emphatically condemn such alliances.

I would now state a few representative facts, merely to indicate the course of inquiry. I shall confine myself to cases of children, born of first-cousins, who appear to be in a partly abnormal state of body or mind. There seems to have been in them an arrest of normal development; they lack that entire and symmetrical unfolding or equilibrium of the physical, intellectual, and moral powers which constitute a *whole* man.

For obvious reasons I shall give only the initials of proper names, and also of towns.

I. Instances of children, born of first-cousins, who lack normal development of *body*.

A physician writes me thus: "I have been a practising physician for more than fifty years, and I have not the least doubt that a large portion of the hereditary diseases which afflict certain families are derived from the intermarriages of close relations; for I have noticed that some families are predisposed to some particular disease; and where families intermarry with those of the same temperament, the strength of that predisposition is doubled."

Mr. C. H., of N., Mass., a shrewd and healthy man, married his first-cousin, an intelligent and healthy woman. They lost three children in infancy; and they have four now living, no one of them bright.

Doctor C., of N. M., New Hampshire, married his first-cousin, Miss B., of U., Mass. They had only two children, and both of them died in infancy.

Mr. J. P., of W., married his first-cousin. Both were in good health, and had active minds. Two sons died before the age of twenty-five; one daughter has diseased eyes; one boy is club-footed; one son died an idiot; and one daughter, who was like common children, married, and at the birth of her first child she died.

Rev. Mr. B., Episcopal clergyman of B., N. Y., married his own cousin; they had two children, both of whom died young.

A gentleman of W., M. County, New York, married his first-cousin. Both these parents were strong and healthy. They had several children, all bright-minded, but each one was a cripple.

N. and S. W., of T., are brothers. One married his first-cousin. Their children are blear-eyed, and feeble in mind. The other married a woman not related to him. Their children are stout, healthy, and bright.

In Adams, Jefferson Co., New York, there are two idiots, born of first-cousins. The head of the eldest, twenty years old, measures but nineteen inches; the other only seventeen inches.

A lady furnishes the following: "Mr. B., of W., married his first-cousin; had two children, both deaf and dumb. Mr. L., of W., married his first-cousin; had two children, both blind. Mr. D., of C. E., married his own cousin; had three children, all hermaphrodites."

In Virginia, where blood relations have intermarried much, in order to keep the property in the family, there are striking illustrations. One statement from a gentleman of Fredericksburg will suffice as a specimen. He says: "A certain family of wealth and respectability have intermarried for many generations, until there cannot be found in three or four of them a sound man or woman. One has sore eyes, another scrofula, a third is idiotic, a fourth blind, a fifth bandy-legged, a sixth with a head about as large as a turnip, with not one out of the number exempt from physical or mental defects. Yet they persevere in intermarrying, although these monuments are constantly before them."

Mr. P., of W., Mass., a fine-looking man, with strong health and good intellect, married his first-cousin, a lady of sound constitution and bright mind. One of their children was club-footed, another was idiotic, another sick and irritable, another near-sighted, another blind, and the rest feeble. Shall we say that these effects are only accidental, where there are such parents?

S. L., of A., married his first-cousin, Miss S. A. They had eight sons and two daughters. Two sons and one daughter are unable to walk. The youngest child is deaf and dumb.

On islands, small and far removed from the mainland, they are apt to marry in and in. A physician from such an island writes me thus: "On the southeast extremity of the island of C. there existed a small settlement of people, who, in consequence of a sharp quarrel with the main island, kept separated from it. The consequence was that they married in and in, till they had nearly all become blood relations; and by degrees they wasted in intellect, and became extinct. Their physiognomy was thin, and with some peculiarities of form. Two were born totally destitute of eyeballs. Their heads were very large, with an abundance of hair that covered their foreheads almost down to the eyebrows. On another island there is a family, the parents are first-cousins. There are four children, three sons and one daughter. The oldest son, who died recently, aged forty, had a peculiar organization: head very large, with hair growing over his forehead, and down his neck; eyes very large, and so constantly rolling that he could not fix his gaze for two seconds upon any object. He could not articulate, his tongue being too large. He had five fingers on each hand, and six toes on each foot. He knew enough to load and draw a hand-cart, and was exceedingly penurious. The second son is no way peculiar. The third son is an incomprehensible being, with an organization somewhat like his eldest brother. He has all kinds of sense, except common; talks much and strangely, and in walking takes immensely long steps. The only daughter partakes of the influence. She has prayed the most of her time during the last ten years. In O. there is a family, parents first-cousins; they had thirteen children, and all, except one, died young. No one lived over twenty years, except the one yet living, who resembles a corpse. On another part of the island is a family, of whom the parents are *not* first-cousins; but they are closely connected by blood, through a chain of intermarriages reaching back several generations. "Within the last ten years," says the physician, "I have delivered the mother of four full-grown boys, three of whom were entirely destitute of eyeballs, although they had very deep sockets for eyes. Their heads were very large, and deformed on the front and top; one exceedingly so. Two of them died immediately after birth, and the other lived but a few hours."

II. Having adduced these facts, as specimens of the many I have gathered, it will not be necessary to add to them a large number which show an absence of normal development in the *intellectual faculties*.

Mr. E. S. and wife, of N., Mass., are first-cousins, both of sound mind and robust health. They had seven daughters and one son. Three daughters are deranged, and the rest are nervous.

Mr. T. A. married his cousin. They had five daughters, some of whom were far from brilliant, and two of them were cripples.

Mr. N. G., from D., New Hampshire, counted twenty-five families within his knowledge where cousins had intermarried, and he says that "not one of their children is above mediocrity."

Dr. H. W. says: "I know four brothers who married cousins, and each has a fool in his family."

Judge C., of H., New York, married his cousin. Out of several children, three were idiots. One only is living, and he has good sound sense.

Mr. E., of M., Mass., married his first-cousin. They have five daughters and three sons. One daughter is an idiot, and two are feeble-minded. One son has run away with the town's money.

A lady from Illinois stated to me the following facts: — "Two brothers married two sisters, and I knew them. The husbands were not blood-relations of their wives. The parties were healthy and intelligent. These families lived near together on a retired country farm, and had few neighbors. There were many children, and all of them had usual health and common understanding. These children intermarried, and the offspring from these first-cousins were very different from their parents or grandparents. Several of the children died young. Of those who survived, some were idiotic, some talked most wildly, and dressed most strangely, and several were irresistibly bent on thieving. There did not seem to be a perfectly normal development among them."

I apprehend that such disastrous consequences as the above could result only where the temperaments of the parents and grandparents were alike. It seems difficult to conceive of such results as merely accidental, and not the effects of fixed laws.

"Mr. and Mrs. E., of M., Mass., had two boys. Everybody called them *dreadful odd*. Their minds seemed topsy-turvy, and they dressed differently from other folks."

Rev. Mr. Wisnor, of Boston, asked a friend: "Do you know Mr. C., who attends my church; he looks queerly, but he thinks more queerly still?" His parents were cousins.

Dr. Pinkney, of Key West, says: "I have seen many inhabitants of the Bahama Islands, the product of the many intermarriages there of blood-relations, and they were generally homely and short-witted." The Doctor further says: "I knew a family in P., New York, twelve miles from G., where the parents were first cousins; they had ten children, all of whom were idiotic."

The "skipper" who took me out on a fishing excursion had a son fourteen years old, who looked and acted as the children of first-cousins are apt to. I asked the skipper if he was married. He said: "Yes; this is my son. I have had many children, but have been very unfortunate with them all." I said no more; but on returning to my lodgings I asked the physician about my skipper's family. He said: "He married his first-cousin, and their grandparents were first-cousins. Most of their children died in infancy, and two of them were born with closed eyes."

There is a man in Massachusetts who married his brother's daughter! They had seven children, all idiots. The greatest age attained by any one of them was eleven years. So devoid of self-care were they, that no one of them could feed itself. They would lie and roll on the floor, and appeared to be only masses of moving flesh. Are not such facts some proof of the violation of natural laws?

I know the family of E. E., of H., Mass. The parents are first-cousins. They have seven children; three of their daughters and one of their sons are idiotic.

Dr. Spurzheim, in the last lecture he delivered in Boston, made this remark: "Among the royal families of Europe who have married in and in, there is now scarcely one of their descendants who can write a page of consecutive sound sense on any scientific, literary, or moral topic."

Dr. Samuel G. Howe, Director of the Boston Institution for the Blind, has devoted his acute and benevolent mind to the investigation of the causes of blindness, idiocy, &c. He communicated a statement to the Senate of Massachusetts in 1848, Doc. No. 51. The following brief abstract is by my friend, Dr. Ed. Jarvis, of Dorchester, Mass., the author of the best American elementary treatise on physiology.

"The parentage of 359 idiots was ascertained. Seventeen were children of parents known to be near blood-relations. Three were from parents who were called cousins. One twentieth of all the idiots examined were offspring of blood-relations. The intermarriages of cousins do not constitute one twentieth of all the marriages, probably not one five-hundredth; therefore the marriage of blood-relations produces more than its share of idiots. All sorts of human deficiencies follow such marriages in similar ratio; namely, deafness, blindness, insanity, rheumatism, excitability, &c. Of the seventeen families where the parents were near blood-relations, most of the parents were intemperate or scrofulous. In some cases, both conditions prevailed. There were born among them 95 children, 44 of whom were idiots; 12 others were scrofulous and puny; one was deaf, and one was a dwarf. In a few instances all the children were either idiotic or scrofulous or puny. In one family of eight children, five were idiotic. Of the seventeen families the account stands thus: —

6 have 1 idiotic child each,	. . .	1 × 6 = 6
2 " 2 " " "	. . . .	2 × 2 = 4
3 " 3 " " "	. . . .	3 × 3 = 9
5 " 4 " " "	. . . .	5 × 4 = 20
1 " 5 " " "	. . . .	1 × 5 = 5
		44

Nearly one half ($\frac{44}{95}$) of their children are imbecile. Add those otherwise imperfect, the 12 scrofulous and puny, the one deaf, and the one a dwarf, then we have 58 in all; thus showing more than one half ($\frac{58}{95}$) in an abnormal condition. The parents who had the four idiots had also four other children who were deformed."

An aged physician sends me the following statement: "A family in the town of B., Maine, named S****, is as follows: the parents have good constitutions, and the usual intelligence; they are first-cousins, and have five children. The first child is healthy in body, and regular in mind; but the other four are miserable idiots. They are disgusting objects to look at, their tongues very large, lolling out of their mouths, and saliva constantly flowing; eyes staring in the most stupid manner; and when they are alone will pick up worms and eat them."

Charles A., of Little Compton, R. I., shot his cousin, Valentine A., August, 1855, when his cousin was leaving Charles's house, after a friendly visit. Charles's father married his own sister's daughter, and the murderer was the product of this marriage.

In Rhode Island two brothers married nearly at the same time. One married a lady who had no blood relation to her husband; the other married his first-cousin. All the parties had good health, and a common share of intellect. Each couple had three children. The children of the couple who were not related were healthy and intelligent; the children of the first-cousins were all idiots. At the birth of the third idiot, its mother died. Two years afterwards the widower married a lady who was not related to him. They have two boys, each is athletic in body, and very bright in mind.

It is not necessary to my purpose to extend these heart-sickening details of human calamity, mortification, and disappointment. With all of them fresh before us, are we authorized to draw conclusions? I think not. It is the part of sound philosophy to suspend decisions until both sides can be fully examined. There are children born of first-cousins who possess full vigor of body, and commanding strength of mind. All such cases are to be carefully registered, and profoundly studied. There are also children born of parents who are not related, who are weak in body or imbecile in mind. These have their place as elements in the present investigation.

With the facts now stated, and others which have come to my knowledge, I would ask the following questions: —

First. Are not the laws (used or misused) which improve or deteriorate the breeds in lower animals, the very same laws of nature which improve or deteriorate the human race?

Second. Are there not found, in the same family, an unusual number of imbecile children, who are born of parents that are first-cousins?

Third. Do any children, born of first-cousins, exceed their parents in bodily strength and beauty, or in mental activity and power?

In closing, let me hope that the Association will not allow the subject to rest till the true conclusions are reached. If members will make note of facts within their knowledge, and forward them to me, I shall feel greatly obliged.

This subject includes an examination of the great fact of *caste* throughout the world. It includes an examination of the South Sea Islanders; of poor Africa, which has suffered through ages from the terrible class-castes, which keep them separated into hostile fragments, thus compelling intermarriages with the same tribe. It includes our American Indians, and may explain in part their diminution in numbers and bravery. It includes the Jews and the Gypsies.

I can assure the student that all these branches will be found full of deepest interest. When the great laws of amalgamation are properly understood, it will not be difficult to show the world how Mr. Knight, of England, has introduced the "Durham short-horned" variety of cattle; or his race-horse, that will leap eighteen feet; or his draught-horse, that walks off with two tons on a level road. It will not be difficult to explain the thick lip introduced into the imperial house of Austria, by the marriage of the Emperor Maximilian with Mary of Burgundy, although it is three hundred years since the event. It will not be difficult to explain the unusual strength and stature of the females of Potsdam, where the King of Prussia had quartered for half a century a guard of gigantic soldiers. It will not be difficult to explain the surpassing beauty of the Circassians and Georgians; or the short stature and small limbs of those parts of Ireland where the true Celtic blood is unmixed; or the stronger and larger limbs of other parts of that country where they have intermarried with English settlers and the Lowlanders of Scotland. It will not be difficult to explain the superior physical and mental qualities of the mixed races in Paraguay, in contrast with their neighbors; or of the offspring of the Dutch in their union with Hottentots. Baron Humboldt says: "It is a well-known fact that the Samboes of S. America (the progeny of blacks and Indians) are remarkable for their physical superiority over their progenitors of either side."

Let the student be assured that all the branches of this wide subject will open before him fields for sublime contemplation and increasing wonder; and when he has exhausted them in other countries, he may then turn to the most interesting and extended exhibition of them all, namely, the United States; and he may try to calculate what will be the solution of the great problem here. Will not our country furnish the most wonderful example of the effects of intermarriages with different castes of the Caucasian race? When the people of these United States become a mixture of English, Scotch, Irish, Germans, and French, will they exhibit a strength of body and an intelligence of mind, a true inborn energy and moral power, which do not equally signalize either of the nations from whom they sprang? Under the fostering care of a truly republican and Christian government, will they advance in science, arts, agriculture, commerce, manufactures, and all the blessings of a religious civilization and political equality, as no one of their parent nations has? Let us hope that it is the appointed destiny of our free and prosperous land, to exhibit a higher development of human attributes than has yet blessed or astonished mankind.

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2. On the Sterility of many of the Varieties of the Domestic Fowl, and of Hybrid Races generally. By Dr. Samuel Kneeland, Jr., of Boston.

The mania which has of late years manifested itself for unnatural crosses in quadrupeds and birds might, if scientifically investigated, lead to many interesting facts bearing upon hybridity. I refer, of course, to *bona fide* crossings of allied and remote species.

A few years since, most naturalists believed that our varieties of domesticated stock, as of cattle, sheep, goats, dogs, fowls, &c., were derived, each genus respectively, from a single wild original; and that man's care had obtained from this the numerous existing varieties. In the present state of our knowledge, we think we are justified in saying that the varieties above alluded to have been produced by the crossing, natural or forced, of several more or less nearly allied species. For instance, who shall dare to decide between the Asiatic buffalo, the European aurochs, and Cuvier's extinct ox, as the undoubted wild original of the varieties of our cattle? Whence the necessity of reducing all varieties to a single stock, endowed with great powers of variation, especially when there are several wild species, each entitled to be considered the original? It would seem that the simplest view of the case is the best, namely, that these varieties are the result of the mingling of many species, guided by the wants or caprice of man. In short, I believe that no *one* wild original can lay claim to the origination of the varieties of our cattle, of our sheep, of our goats, of our dogs, of our barn-yard fowls; — and, to carry the opinion to the legitimate consequences, that no *one* species of MAN can lay claim to the paternity of all the *human* varieties. The reasons for this opinion have been often stated, and will not be repeated here; they may be found in the writings of Hamilton Smith, Morton, &c. Some new observations will only be added in confirmation. And yet, with this belief in the diversity of origin of our domestic animals and the human races, it seems to me that *hybridity* is a true *test* of *specific difference.* It is an axiom with some, that *different species* will not produce fertile offspring; and hence, to them, the fact of the production of such offspring proves that the parents belong to the same species. On the other hand, Dr. Morton makes different *degrees* of hybridity, the offspring being more prolific according to the nearness of the species; thus making hybridity *no test* of specific difference. Of these opposite opinions I prefer the first. By a hybrid race I do not understand an offspring prolific for a few generations, and then gradually dying out, or feebly supported by crossing with the original stocks, but a race capable of propagating itself without deterioration, without any assistance from either of the parent stocks. Such a race, I maintain, has never been seen, and never will be, under the present laws of animated nature. You may take any part of the animal scale, from a barn-yard monster to a mulatto, and the fact is the same, — they cannot hold their own; they must die out unless crossed by the pure originating blood, or must return to one or the other of the primitive stocks.

The subject which suggested these remarks is the sterility and deterioration of some of the highly-bred varieties of our domestic fowls.

It has become quite a source of complaint by many farmers, who in former times had plenty of eggs from a small number of common fowls, that, since the mania for rare breeds, they have been unable to obtain their usual supply of eggs and chickens from the same number of birds, and have been subjected to great annoyance and pecuniary loss. It is a natural consequence of forcing birds of different origins, and from different countries, to propagate a hybrid offspring, for this very reason prone to degeneration. The difficulty is increased by the impossibility of crossing the stock with the supposed originals. The size of the bird, in this as in other cases, seems to be obtained at the expense of the reproductive powers. The admixture of different species, and breeding "in and in," have been carried beyond the limits fixed by nature, and deterioration is the result.

The same thing occurs in the mulatto, and other hybrid human races. The mulatto is often triumphantly appealed to as a proof that hybrid races are prolific without end. Every physician who has seen much practice among mulattoes knows that, in the first place, they are far less prolific than the blacks or whites; — the statistics of New York State and City confirm this fact of daily observation; — and, in the second place, when they are prolific, the progeny is frail, diseased, short-lived, rarely arriving at robust manhood or maternity. Physicians need not be told of the comparatively enormous amount of scrofulous and deteriorated constitutions found among these hybrids.

The *Colonization Journal* furnishes some statistics in regard to the colored population of New York City, which are painfully interesting to philanthropists. The late census showed that, while all other classes of our population, in all parts of the country, were increasing at an enormous ratio, the colored were decreasing. In the State of New York, in 1840, there were 50,000; in 1850, only 47,000. In New York City, in 1840, there were 18,000; in 1850, only 17,000. According to the New York City Inspector's Report for the four months ending with October, 1853, —

1st. The whites present 2,230 marriages; the colored, 16.

2d. The whites present 6,780 births; the colored, 70.

3d. The whites present about 6,000 deaths (exclusive of 2,152 among 116,000 newly arrived immigrants and others unacclimated); the colored, 160.

Giving a ratio of deaths among acclimated whites to colored persons of 37 to 1, while the births are 97 to 1. The ratio of whites to colored is as follows: — Marriages, 140 to 1; births, 97 to 1; deaths, 37 to 1.

According to the ratio of the population, the marriages among the whites during this time are three times greater than among the colored; the number of births among whites is twice as great; in deaths the colored exceed the whites, not only according to ratio of population, but show 165 deaths to 76 births, or 7 deaths to 3 births, more than two to one.

The same is true of Boston, as far as the census returns will enable us to judge. In Shattuck's census of 1845 it appears that in that year there were 146 less colored persons in Boston than in 1840, the total number being 1,842. From the same work the deaths are given for a period of 50 years, from 1725 to 1775, showing the mortality among the colored to have been twice that among the whites; of late years Boston probably does not differ from itself in former times, nor from New York at present.

In the Compendium of the U. S. Census for 1850 (p. 64), it is said that the "declining ratio of the increase of the free colored in every section is notable. In New England the increase is now almost nothing"; in the Southwest and the South the increase is much reduced; it is only in the Northwest that there is any increase, "indicating a large emigration to that quarter."

What must become of the colored population, at this rate, in a few years? And what are the causes of this decay? They do not disregard the laws of social and physical well-being any more than, if they do as much as, the whites. Prominent among the causes of decay is the continual attempt to mix races, — the hybrids cease to be prolific; the race must die out as mulatto; it must either keep black unmixed, or become deteriorated, and finally extinct. Nobody doubts that a mixed offspring may be produced by intermarriage of different races. The Griquas, the Papuas, the Cafusos of Brazil, so elaborately enumerated by Prichard, show this; the question is, whether they would be perpetuated if strictly confined to intermarriages among themselves. From the facts in regard to mulattoes, we say unquestionably not. The same is true, as far as has been observed, of the mixture of the white and red races in Mexico, and Central and South America. The well-known infrequency of mixed offspring between the European and Australian races led the colonial government to official inquiries; and to the result, that in 31 districts, numbering 15,000 inhabitants, the half-breeds did not exceed 200, though the connection of the two races was very intimate.

A recent opportunity of witnessing the landing of a large colored picnic party afforded the most striking proof of the inferiority and tendency to disease in the mulatto race, even with the assistance of the pure blood of the black and white races. Here were both sexes, — all ages, from the infant in arms to the aged, — and all hues, from the darkest black to a color approaching white. There was no *old mulatto*, though there were several *old negroes*, and many fine-looking mulattoes of both sexes, evidently the first offspring from the pure races. Then came the youths and children, removed one generation farther from the original stocks, and here could be read the sad truth at a glance; — while the little blacks were agile and healthy-looking, the little mulattoes, youths and young ladies, were sickly, feeble, thin, with frightful scars and skin diseases, and *scrofula* stamped on every feature and every visible part of the body. Here was hybridity of human races, under the most favorable circumstances of worldly condition and social position; and yet it would have been difficult, and, I believe, impossible, to have *selected*, from the abodes of crime and poverty, more diseased and debilitated individuals than were presented by this *accidental* assemblage of the victims of a broken law of nature.

Such facts as these lead to the belief that hybridity is a true test of specific difference; and that admixture of species, in man or animals, must end, sooner or later, in deterioration and extinction, — very soon if unmixed with the pure stocks, and later if thus mixed; that, at any rate, extinction is the doom of the hybrid race, either by absorption into the parent races, or by slow decay.

For wise purposes, which we can know but imperfectly, the Creator permits, as we see, different races of men and animals to produce fertile offspring *inter se*. Thus far, and no farther, can man go in his attempts to mingle species, which change perceptibly within narrow limits, and then perish, or return to their origins.

One of the consequences of this opinion is, that the *genus* HOMO consists of several *species*.

3. CONTRIBUTIONS TO THE PHYSIOLOGY OF SIGHT. By DR. T. C. HILGARD, of Philadelphia.

I. *Accuracy of Sight and Means of Accommodation.*

IN both these subjects important points are still enveloped in doubt, partly from a want of explanation, partly from indistinct or equivocal conceptions; but chiefly from a want of sufficient experiments, and the effect of others which, by equivocal results, have produced only more obscurity. It may, therefore, not appear superfluous to introduce some original observations and experiments tending to elucidate those points in a connected exposition of the whole subject, in order to show their bearing, and to bring a number of known facts under their proper point of view, without, however, any pretensions to novelty.

The simple considerations must necessarily precede the compound, — the consideration of monocular that of binocular vision.

However far an eye can reach, it is impressed distinctly only with a single distance at a time, all objects more remote or near appearing for the moment ill-defined or vague, while they may be rendered distinct in their turn, to the disadvantage of others. This adjustment is known to be subject to volition. The eye is supposed to adapt itself to suit given distances, which faculty is called that of *accommodation*. The range between the farthest and the nearest distances of well-defined vision is the *scope of sight*. In near-sighted (*short*-sighted) eyes it embraces only a few feet. In normal and far-sighted ones it can extend to celestial distances. The two eyes differ mostly in this respect: the short-sighted one may, for example, see accurately from four inches to six feet distance, the normal one from three inches to infinity.

To see parts as *minutely* as possible, we must bring them as near as possible, the angles then being the largest. Experiments, however, seemed to show that *perfect accuracy* of sight took place at a distance different from that, and that it also had a certain *scope*, in near-sighted eyes, nearly coinciding with that "of vision" generally.*

The limits both of visual range and of *perfect accuracy* (not a very

* In normal and far-sighted ones, however, "exceedingly variable, even extending as far as 500 feet." (Budge.)

good distinction, as the latter seems to be included in the very definition of the former) are determined by *optometers*, which are graduated bars, with a sliding test-object, to be stopped where it begins to be indistinct. Thus, for establishing the scope of *well-defined* vision, the point of an acute corner is used in an object plainly visible as to size, the point being produced by the meeting of two rectilinear margins. It is set up at a distance, where it has as good definition as the eye is capable of. It is moved toward the eye by degrees, affording the organ a little rest at each station; suddenly a point is arrived at where the points and margins become *diffused* by a spreading dimness. Thus, a *nearest* and a *farthest* limit are obtained, if an infinite distance too can be called a limit. For the determination of the extent of minute vision *print* is generally chosen, which is moved to and fro so far as possible without becoming *inconvenient to read*, — a circumstance greatly dependent on external circumstances, such as illumination, and chiefly *size*.

It is evident that, in seeing with both eyes at once (binocular vision), when it is required that both eyes be *accommodated to the same distance*, and that their axes *meet* at that distance, the impressions of both eyes being then duly superincumbent in our consciousness, the range of defined as well as minute vision cannot extend beyond the nearest one of the outer limits of the single eyes, nor this side of the farthest inner limits, that is, *cannot extend beyond where the eyes can operate in common.*

It is also evident, that, in experiments on visual accuracy, all artificial inconveniences, and chiefly all *exclusion and diffusion of light*, as far as not founded within the organ itself, must be carefully avoided. This fundamental requisite, however, we find entirely disregarded by most optometrical contrivances, by being complicated with the *Scheiner* experiment. The eye is constructed after the plan of a camera obscura, and accuracy of sight obtained by focality of the *retina*, the nervous expanse in the rear of the camera and lining the eyeball. The idea is to make supervening extra-focality more perceptible by preclusion of the more central rays, thus causing an incipient or total separation of retinal images, as soon as homocentric rays are no more unitable on it. A screen is placed close to the eyes, with two stitches or clefts in front of the pupil, but so as to have its centre screened, and only a few lateral pencils of light admitted, each producing a dim configuration of its own representing the lucid object, — as in an occultation of the sun, when its orb is left crescent-shaped, each chink in the foliage will project on the ground a crescent-shaped luminosity, while a full sun projects round specks, and a candle shining through pin-holes as many inverted representations of itself as there are chinks. These, if collected by a lens, will form a single image in its focus, but forward and backward of it as many separate ones, gradually seceding as they recede from the focus.

Nothing so quickly affects the eye morbidly, and causes rapid destruction of nervous eyesight, as *diffused rays*, such as are caused by the proximity of a screen's surface, in the first place. In the second place, this contrivance, in precluding the central rays, precludes the only rays that can be united into an exact focus, owing to the spheric form of the lens; thus in no case can an exact image be obtained, even if focal. Thirdly, the intensity of the light normally admitted through the pupil is reduced perhaps to its hundredth part, the pencil of incident light being thus reduced. Fourthly, the inflection of light passing by margins, or through chinks (condensation of air along surfaces ?), causing a considerable aberration, will never permit of an exact image being produced.

Hence, the complication with the *Scheiner* experiment not only leads to incorrect results as to scope, but furnishes a multiplicity of results that belong to quite a different class of optic phenomena.

In experiments on *scope*, as well as *minuteness*, the objects must be perfectly *open to sight*, and duly illuminated.

In experiments under conditions entirely plain and natural, the moment the sliding point in contemplation passes the limits of accommodation, that very moment it becomes *ill defined*, clearly enough so to be at once observable. If brought *near the proximal limit*, that indistinctness can be contemplated as such, and studied in its form and nature *with paramount attention*, without producing the slightest change, except that the *ideas* concerning it flow more freely. Nor does the *wish* to see more clearly for itself produce any sensuous change. It is only an *exertion of will to that purpose*, — an *exertion*, not a mere wish, and a very sensible exertion, — that is able here to produce accommodation. This exertion is clearly felt as a *muscular one*, and occupying the whole orbit except the middle filled with the eyeball, thus *exactly corresponding to the position of the external eye-muscles.*

In accommodation for distance, on the contrary, the greatest feasible *relaxation* is experienced. If the *muscles* with which we turn and move the eyes be wilfully relaxed to the feasible extreme, not only the eye-axes assume a parallel position, as in *adaptation for infinite distance*, but at the same time each eye is *accommodated* for infinity. If the muscles that move the ball be wilfully "strained," contracted as much as possible on all sides, then the eye is found to be in accommodation for greatest proximity.

These experiments are accessible to every one, and can be realized without an apparatus. I think it a most important support for the theory, that *all sensuous exactness*, also in the eye, *is produced objectively, and as far as voluntary by muscular action.*

Besides certain universal, mostly unconscious, but often voluntary, irradiating effects from person to person, and strictly dependent on and expressive of *psychic* conditions, the notorious manifestation of the mind on the material world is *muscular action*, namely, by a *material exertion of will.* A mechanical effect of the mind is thus produced, but also a *sensuous* one, producing a voluntary correctness or indistinctness of perceptions. In the four lower senses these effects, too, have been proved to be *mechanical* ones, *owing to muscular action*, by rendering the form of the ultimate sensuous *stimulus* more defined. The exertion of will to *hear* more distinctly affects the *musculus tensor tympani* to span the tympanum, whereby better-defined vibrations are produced, that come to a final perception accordingly. The *olfactory* perceptions are regulated by the modes of inhalation and mastication (see Proceedings of 1854, p. 251), the *gustative* by the latter; while the recognition of qualitative diversity among the impressions once perceived is a subjective function much dependent on *attention.* In sensuousness we have two psychic functions: a passively sensuous one, *perception*, and an active intellectual one, *recognition* or judgment, which, being of the intellectual order, is affected by attention.

We have, therefore, every reason to expect that distinctness of vision, too, is owing to *physical* conditions, in the first place, and secondly, as it is influenced by exertions of will, to *muscular* action.

It was evident that distinctness of vision depended on distinct representations on the retina; at least this idea was not to be left, and recourse taken to inherent psychic faculties, before all possibilities of a physical nature were properly explored (*exploitées*) and exhausted.

We must here make a few remarks on the proper method of physiological investigation, as by a want of it physiologists were so frequently misled to skip their best and due chances of a proper understanding, running their boats directly ahead into the shoals, instead of following the *channel*, floundering upon *instabilis tellus*, *innabilis unda*, when they attributed distinctness of sensation to *attention*.

All sciences consist of two parts: *the empirical mass and matter of facts*, and the *understanding* thereof, or knowledge of relations ("the humor of it,"—Shakespeare). In natural sciences this is self-evident, as the empirical mass is so much more ponderous than the ideas concerning it. Even in mathematics, if no objective casualties had led to various researches of relations (as each one of unknown magnitudes requires an independent equation or concrete occurrence), its system would never have been generated. Its present form is an accidental one. It is not complete. Who can tell how to complete it? No science was originated *a priori*, but *a posteriori*, starting from concretes or casualties.

The empirical is the body scientific; the understanding, the soul thereof. The empirical is the reality to operate upon. Understanding is the comprehension of the unknown by the known. We must in each case proceed from the *most known*, *most real*, *most absolute*,—this is, no doubt, the fundamental rule. The second is to skip no link of the chain, and to linger at each until all combinations are exhausted.

For us there are two different, though corresponding, realities or empiricisms. But few connecting links are known. One is *sensuous* empiricism, which brings the impressible but blank mind to the knowledge of a reality not founded within itself, the "outward world"; the other is the *mental* empiricism, which takes cognizance of its own inherent conceptiveness and energies, its intellectual and moral processes and results, to which sensuous empiricism furnishes the material to digest.

As the becoming conscious in our sensuous energies depends entirely on *material casualties*, these are to be considered as the conditioning causes, the *empirical domain* or reality of sensuosity, and therefore we must, in the explanation of sensuous phenomena, *commence* at the most extraneous, as the most conditioning in the concrete cases; but in the determination of the intellectual, and its effects on the physical world, at our *mental* consciousness, each in its turn being the most primal or fundamental.

A real explanation requires to be constituted of facts and *pre-established* conceptions; otherwise it is no understanding of unknown by *known* things. If not so, they only appear as hypothetical postulates, each hypothesis remaining to be proved.

Thus is it with the assumption of *attention* as a factor in *sensuous* phenomena. *Attention* we meet on the empirical field of *intellect*, not of *physics*. It is in the *intellectual* range that we fall in with it, that we experience and conceive of it. Here it is that its properties must be studied. If the properties thus established can explain anything in the sensuous way, so much the better. If the study of each conception, sensuous and intellectual, be commenced at its *proper source*, if at all conductible so far as to meet, they are sure to *meet at the right point*. In what relation attention stands to the sensuous, namely, indirectly by steadying the conceived will, we shall presently see.

From the proper manifestations of attention, I think it cannot be more plainly qualified than as *the faculty of the mind to persist in a direction once assumed*. It is, as it were, *the momentum of the mind*, the *persistence of animadversion*.

Wherever the mind is *fixed* upon, thereunto the ensuing thoughts *e*volve and *re*volve. This power of attention in intellectual matters being so well known, the temptation was great to ascribe to it a similar efficiency and productiveness in sensuous phenomena. It exercises its influence on will and judgment, but not on sensation itself. The explanation of sensuous accuracy by attention must therefore be rejected, as, —

In the first place, satisfactory explanations can be furnished by *physical* causes dominating the final object of perception.

In the second place, attention has *no such power pre-established*, which would at least remain hypothetical; if not

Thirdly, the physical experiment *disproves* attention to be at all directly influential, except by actuating the *mind* to ideas and will-energy. We may hear sounds as distinct or as indistinct with equal attention, and distinctly or indistinctly without attention, the impression on the sensorium remaining the same. We may read with eyes or hear a whole page distinctly, without becoming conscious of the *sense* conveyed, if attention be absent. Returning, it may cause the mind to peruse the last impressions meaninglessly impressed on the sensorium, the last words, the strokes of a clock. Sensuous recollection will repeat the words, the strokes, such as they were, and the now attentive mind may conceive of a meaning in the words, and of a number of the strokes, &c. Indistinct vision, as we have shown, can be "*attended to*," and conceived *as such* with perfect attention, without thereby becoming more distinct.

Physical distinctness was to be sought for. Being voluntary, it was to be sought in the organ. It was to be sought in its *stimulus*, light. Light presented distinctness in the focus. The focus was found on the retina. The retina was found sensuous to light.

Olbers computed a variability of $1'''$ of distance between the lenticular system and the retina to answer all purposes of accommodation. Recent experiments have shown the difference of focus in (dead) human eyes, for objects from 4 inches to infinite distance, to be about $1'''$.

What could produce this requisite volubility? The most fabulous and directly untrue explanations were given, facts asserted that do not exist, and credit given from "want of a better," or rather from mental laziness.

Within the eye, *in loco*, no original motive causes can be adduced. The *cornea* (the transparent outward convexity) does not change. Next comes a fluid. The lens changes its position *quite actively*, and in accommodation for proximity its surface has been observed to come forward.* There are no voluntary organs of motion within the bulb, only involuntary muscular fibres subservient to the reflex action of the *iris*. A circle of half-gelatinous muscular papillæ at its insertion and base (*corona cillaris*), projecting not more than at most $\frac{1}{10}$ of a line, was put in requisition to "press the lens backward," in order to produce accommodation for distance, against the united stress of four powerful lengthwise muscles; the "*straight*" ones, fixed at the very recess of the orbit, and spanning their flattened sinews over the globosity of the elastic ball, ending near the cornea; and two transverse ones, forming a tractile *belt*.

The most plausible, and probably the only possible explanation,

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* The writer is at present engaged in a series of experiments on that subject with *obscured lenses* and *pupillar adhesions* in living eyes.

was for a long time quite neglected. The bulk of the ball, between the retina and the lens, which it bears in front suspended in a strong transparent diaphragm, is occupied by a toughly gelatinous elastic mass (*corpus vitreum*). A pressure of muscles acting always on it around its "equator," it *cannot help yielding* to their various degrees of tension, as the bulb cannot move backward, being sustained in position by a fatty bolster behind and laterally. It must yield in the free direction, must *elongate*; hence it *cannot help to produce various accommodations*, elongation being required for accommodation to proximity. Relaxation must shorten the intra-ocular distance, *does* accommodate for distance. The *corpus vitreum* being elastic, it must yield in the axial direction in both senses; if the retina be swelled backward $\frac{1}{2}'''$, elongating the bulb $\frac{1}{2}'''$, the lens may also be moved forward $\frac{1}{2}'''$, and the total difference is $1'''$, as required.

The accuracy and delicacy of changes is obviously explained by the disposition of the forces, a great contraction of a muscle producing only a small lateral effect on the bulb over which it is bent.

We have stated before, that all *exertion* to effect accommodation is felt to be a muscular one in the locality of these outward or voluntary eye-muscles. Also, if those muscles be wilfully contracted, then the eye is really accommodated for proximity. Also, if we *rest* and *relax* the eyes, they not only assume the parallel direction, but are accommodated for distance. Every one can thus practise wilful accommodation.

I mention these facts in corroboration of this theory of accommodation, held and cultivated by Von Graefe (at Berlin). Whoever may have been the first to originate it, he proved it conclusively by the observation of the effects of morbid and artificial paralysis of the external eye-muscles, the latter ensuing directly on an application of the *neutral sulphate of atropia*, long before any of its effects on the iris and interior parts take place. The muscles being relaxed, the eye stands out and is accommodated for an inalterable distance, namely, farthest distance; — whatever be the wishes or "attention" of the patient, the organ to execute his will is paralyzed!

The conditions of the *distinctness or indistinctness* of sight, and the degree of accuracy, depend, —

1. On the fact that only the central part of the retina, situated where in looking straight forward the optic axis is incident, is organ-

ized to perceive *forms* distinctly. The eccentric parts are less delicate in form-perception. Images laterally incident are less distinct.

2. On the focality or extra-focality of the central part of the retina. That part being out of focus, it perceives distinctly the inaccuracy and diffusedness of the images. If, by a slight distortion of the bulb, as in *turned* eyes, it changes its position toward the optic axis, the intense light of that axis will be incident obliquely, and hence diffusedly, all points except one being out of focus, and produce a less distinct, but a far more fervent perception.

3. The gelatinous body (*corpus vitreum*) in a normal state offers no conditions, except that its refractive power, like that of all the refractive mediums, influences the distance of focus and correction of light-dispersion. Also, all morbid conditions of the several organs, infiltrations, exudations, hœmorrhages, &c., in short, all material lesions, interpositions of opaque bodies, morbid changes in the refractive powers, and the sensitiveness of the nerve, are influential, but are here not further entered upon, as being abnormal.

4. The irregularity in the lens, causing luminous points (stars, lanterns) to appear *beamed and pencilled*, &c., and resulting from the peculiar arrangement of the fibres around two amorphous Y-shaped central configurations, an upright one in front, and an inverted one behind, producing the appearance of a six-rayed star, — in the lens to the looker on, in the sky to the looker *out*. Of course the *size and intensity of these diffusions of light*, and their changes in deflected-looking, *constitute a minimum limit of accuracy*, outlines being diffused to a certain extent; hence figures whose retinal image is within the extent of that aberration will be no more distinguishable as to form. Thus objects of a size insufficient to subtend a certain visual angle are absorbed in the irradiation. In what relation this dispersion stands to the focus and its distance from the lens, hence to accommodation, has not yet been ascertained, to our knowledge. The unsteady accommodation of the short-sighted eye, that cannot adjust for parallel rays, in binocular contemplation of a star will produce a certain vacillating contingent of its apparent "*rays*," while other and most eccentric beams are superadded by the capillary attraction of moisture along the pinched or quivering eyelids.

5. The spheric form of the lens causes a spheric aberration, the rays passing through lateral parts not becoming united in a perfect

focus. This alone would produce a certain mellowness of outline, and hence a minimum limit of visual angle; but its effect is much smaller than that caused by the starry irradiation of the lens, resulting from its structure. If, however, the *pupil* be considerably enlarged, as in looking into the dark, the effect is very sensible, increasing the extent of the irradiation, and by falling on eccentric parts will cause great luminosity and fervency of color; hence,

6. The width of the pupil is of great importance. In the dark, and in accommodation for distance, it enlarges, admitting of a greater quantity of light. In the latter case, without any detriment to accuracy, spheric aberration being the less considerable, the less divergent the rays, the farther the object of vision. The yellow light uses and pains the eye the most, causing a stronger contraction of the pupil, and less admission of light generally. If by a screen, spectacles, or a chimney of true-blue (e. g. cobalt blue) glass, much of the yellow light be precluded, the expanded pupil admits of so much more light that the increase of quantity more than compensates the loss of certain rays, and vision is actually brighter through such glasses that are really somewhat darkening.

The *humor aqueus*, — the fluid between the lentine diaphragm and the *cornea*, — as well as the latter, does not in a normal state present any conditions for dimness of sight.

7. The lachrymal fluid streaming down over the eye, and the margin of the eyelids where it accumulates by capillary attraction, has been mentioned before.

8. The direction of the eye-axes in binocular vision. If they fail to meet (converge), as in muscular debility (Von Graefe) by overstraining, so frequent in seamstresses (" dulness of sight "), as well as for points before and behind the distance of converging axes, *double vision* exists. (Collateral correspondence of retinal parts, the right side being perceived as coincident or " identical " with the right, the left with the left side of the other ball, is obtained by rays from identical points at the distance of converging axes.)

9. The difference of accommodating power or scope of sight. A co-operative eye, unable to accommodate for the distance for which the other is adjusted, will receive only a diffused image, and trouble the clearness of the other. Hence distinct binocular vision cannot exceed the scope between the farthest one of the proximal and the nearest of

the remote limits afforded by a pair of eyes as above mentioned. In two eyes co-operating, every want of accuracy in one will injure the correctness attainable through the other singly.

10. All the outward conditions influencing the state of the arriving light, as aerial dispersion and vacillation. On alpine heights, through a serene and homogeneous atmosphere, thousands of feet above the dusky atmospheric ledge overspreading the lowlands, on snow-fields 40 miles distant, "small type" executed in letters of 1,500 feet would be read as easily as that of $\frac{1}{16}$ of an inch on a page at 8 inches' distance, which in its turn would cover towering summits, as that of the Toedi, seen near Zurich, in Switzerland, the angle being the same. Sizes increasing after the same ratio as light decreases in intensity,— namely, after the ratio of expanding angle or rectilinear emanation,— distance itself being without influence as to intensity, and as to aerial dispersion, the loss is perhaps more than compensated by the enlargement of the pupils.

Celeris paribus, vision is minutest under conditions where angles become largest, namely, at the greatest proximity that can be accommodated for, and this is proved by experience. But that adjustment requiring a great exertion, and exercising the greatest pressure on internal parts, it can be sustained only for a very short time. The muscles become tremulous, tears flow, the eyes ache and become dark, &c. For a sustained minute vision a greater distance is chosen, some of the angular size being readily sacrificed to comfort, and a certain empirical distance for close work is habitually assumed.

We have thus for each single eye as to *accuracy*,—

1. *A scope of well-defined vision* equal throughout, provided the angles under comparison be the same, and allowance be made for external obstacles to light.

2. *A distance of minutest vision*, being the proximal limit of accurate vision generally.

3. *An habitual scope of minute vision*, much dependent on external conditions, and more distant than the proximal limit of accurate vision. To such very variable distances of comfortable minute-sight the images of unknown distances are referred, to reading-distance in a microscope, say 8 inches. The culminating sun and moon, if considered half a foot in diameter, as they mostly are, being $\frac{1}{2}°$ of angle, are put at about 60 feet height, the height of trees and houses; if considered 2

or 4 feet diameter, as the rising moon sometimes is, at about 120 or 240 feet, — distances from which we *scan* such buildings and houses. In a telescope, the moon, though magnified and more distinct, is sometimes imagined as the contingent part of the objective glass (the eye-piece being too near), or is set at the reading distance of common type. Astronomers are apt to assume no definite idea of telescopic size at all.

4. A *degree of acuteness* or accuracy, or a minimal angle, dependent on the aberrations within the organ.

In binocular vision allowance must be made for the peculiarity of each eye, and their co-operation, the effects of either being mutually superincumbent.

II. *Visual Judgment of Position.*

We realize the existence of space by movement, made conscious by resistance in space, through touch. The sensation of muscular exertion is one of touch, namely, of a dull pressure in all the muscles employed; hence the more expanding and diminishing, the farther from the point of resistance. Thus we know of locality by touch, because perceptive of *resistance.*

Taste teaches nothing of locality, except by effects of pain or contraction (pungency, acridity, astringency, — see Proceedings, 1854, p. 249). The lingual sense, as well as the olfactory and auditory, teaches us nothing about space, only about specific sensations. It is only touch and pain, and the latter hardly otherwise than as far as determinable by touch combined with motion, that teach us about the localities of the other specific sensations. We think we taste everything in the mouth, where we have organs of touch and motion, while the greater part of it, the "*peculiar flavors,*" are perceived at a few inches' distance above it, in the nasal organ. Olfaction, however, though essentially the same as flavor-tasting, we place pretty correctly, as we *feel* the streaming coolness of the air. Yet we place it about an inch too external, because the cool air and the fingers do not reach farther, and it is they (touch) that give us locality. Things taste "delightful to our palate"; while tasting we move the gustative tongue along the (hard) palate, which itself has neither a gustative nor an olfactory perceptiveness, but only that of touch or localization. We do not know where we *hear* the sounds. By motions (positions of the head) we ascertain from the greater or less intensity of sound whence it proceeds, but not where it is perceived. A soreness at the faucal entrance of the *eustachian tube* (connecting with the middle cavity of the ear), we place in the external ear, near the tympanum, just beyond what we can *reach* with the tip of the finger. Each organ has a tactitive apparatus: the tongue its lips and muscles, &c., the nose its tip and skin, the ear its conch and the flap thereof, — pretty sensitive are those appendages as lips and tips, real bolsters of taction, — and the eye has its lids, lips as it were, its very sensitive external surface, and its muscles, all three constituting a very delicate organ of touch, and hence of locality, harmonizing with all ideas of space acquired by the proper organs of localization, the extremities.

The internal parts of the eye have no tactitive sensitiveness. If the optic nerve be pinched, it answers by a flash as of lightning. The retina, if touched by the pressure of a finger on the ball, answers by a ring of light. It is a *local* effect. Still the perceived light is placed at several inches' *distance*. Therefore our visual ideas about position are only a function of *judgment*, an operation of the *mind*, not of the eye; and the mind refers it so as to harmonize with its acquired ideas of space as given by taction. Whereto the muscle must pull and the *conjunctive* (the sensitive external membrane of the eye-front) feels the lids gliding along, and whereto the fingers may grope both the object and the direction of the ball, thereunto is the contingent visual impression referred. How we would refer without taction is a vain question, for we *have* taction, and *know of space*.

In handbooks of physiology, even in monographs, we almost invariably meet a wonder expressed how it is that we "see things upright," whereas, the eye being constructed like a *camera obscura*, a reversed image is projected on the retina, — "and it is this picture we perceive." Here is the error. The idea of the *picture* consists of *two* things: of light-impressions and of relations of space (forms, &c.). To perceive light is the department of the visual nerve. This is what we *perceive* of the "picture." To conceive of *form* therein is the part of judgment by reference to space-consciousness, touch. We evidently do not perceive *that* picture at all, for we are in no way subjectively conscious even of the existence of our retina, much less of its size. We continue to feel even as the child does; our looks *travel* out, and glide over the surfaces we view, penetrate into distances, sweep, return, — all of which are ideas of *taction*, the more so as any *exertion* towards the visual determination of distances — parallax — is felt as one of *touch* (muscularly). The exertion to accommodate to distances is the effect of the same muscles, and the turning in directions *per se.* The *co-occurrence* of certain *sensations of light* with certain *ideas of location* being infallible, and the real connection subjectively unknown, we imagine light-perception to be localizing or tactitive, referring two facts in causal connection directly together, just as any two points can be connected by a direct or straight line. It holds good until the moment where we affect the retina by touch: here we see an incongruity between vision and touch, one that, be it well remarked, does not alter in the least our ideas about the location of either the finger or the organ, but of the light-effect, — we place it outside. If we press the outside of the ball, the fiery ring will appear to be beyond the nose at four or five inches' distance, and coincident with a ring produced by pressure on the (correspondent) inside of the other eye, beyond the nose, placed at one or two inches' distance from its actual locality, at the very point where an object must be put to make light incident on these two corresponding parts, provided the nose would not interfere, which it does however, showing that, even where they cannot possibly co-operate, two retinas are made to "correspond," after the general plan. Each eye refers light where touch has taught us light *proceeds from.* If persisting awhile with closed eyes, one almost succeeds in bringing the two sensations in *direct* connection, localizing the ring unto the touch, but never referring the touch to the distance the ring seemed to hold.

That to the *retina* all references to space are only acquired and not absolute, is easily proved by the fact, that all these relations can be trained to their very reverse, while in no case a continued incongruity between our present and former spacial consciousness and actions (motions, &c.) takes place. Dressing before a mirror, we change our visualrelations to motions; right becomes left, &c.; and if you comb your hair backward, it seems to go absolutely forward in the glass. Still the practice of reversing the relation is so soon acquired, that we do it unconsciously. On turning away from the instrument, whether glass, microscope, or telescope, we have only to reverse the practice of our eye-muscles in relation to the others, which makes the eye spin for a moment, and their instability causes vertigo, an incongruency between the tactive effects of gravitation and a customary relation of one set of muscles to the others. Resistance caused by gravitation regulates our ideas about up and down, the only fixed starting-point of space we have. Thus the eye, as a whole, is not only a visual, but also a tactile organ, in this combination more delicately so than the rest, *localizing* the sensations of its *visual* apparatus. The *specific* sense of the eye is *luminosity*, but *sight is a co-operative function of two, as taste is of three senses.*

Blind-born persons acquire a most accurate tactile knowledge of space. On receiving eyesight, they immediately refer visual impressions as we do, and upside remains safely upside, because all light-impression is localized by application to touch.

That we have in our retina no actual or reversed knowledge of upside and downside is proved by standing on the head, thus reversing the retina, while our ideas about up and down remain exactly the same.

If we had any knowledge of *actual size on our retina*, its impressions would be in a perpetual conflict with actual sizes of *touch*, which is not the case. That we have not is also shown by the experiment of looking through a magnifying lens, and drawing with the other eye an outline to coincide with the magnified image; although *coincident*, hence equal in angle and retinal extent, still the sketch may seem three times larger, because three times nearer than the habitual distance assumed in the microscope. The two impressions are not recognized to be equal in size, although their "pictures on the retina" are, one factor of our judgment of size, tactile distance, being different.

Hence the only faculty we need assume for the retina is an empirical knowledge of the relative retinal subtensions of angles, or power of empirically identifying certain retinal distances (sizes) with certain differences of direction taught by touch. This would suppose that in the organ certain *directions* were in immediate relation, or, as it were, identification with certain *points of the retina.* This would require organs in immediate connection with the retina, each having a direction of its own, and by it a physical action on the part in question, so as to refer to it individually. It would require millions. *And so we find it:* the bottom of the transparent retina is paved with myriads, as it were, of parabolic mirrors of very minute size, their mouths toward the centre, their axes in radial positions. It is the deepest or most peripheric stratum of the retina itself, the so-called "cylindric bodies," conoid crystalline bodies severed from their darkly pigmented matrix by a coating of a yellowish oil, which constitutes the concave mirror. Some have claimed these crystalline elements as the seat of luminous sensitiveness.

Thus an identification of retinal parts with directions and their differences, angles, may be acquired. This done, sizes may be judged by the application of angles to tactile (feeling and convergent) distances, and, *vice versa*, distances by the angles of sizes tactually ascertained, which would seem sufficient to explain all the phenomena in question.

III. *On Subjective Color-Effects, and their Relations.*

Every impression of light or color disposes the eye to produce the sensation of its complementary light and color as soon as withdrawn from the former influence. Gazing in the direction of a red surface suspended on a white wall, when the red sheet is dropped, a green configuration appears in its place. Yellow produces lilac or purple; blue, orange; and *vice versa*. The *phantom* or subjective complementary effect will endure some time, according to the strength of the impression. Dwelling on a colored surface, namely, perusing it in divers directions, the impressions along the margins will be alternately mixed with the adjoining colors, while the impression of the centre remains uniform, hence becomes most intensified by duration, and therefore will produce the most enduring complementary effect; hence the phantoms of a uniformly colored surface will commence vanishing by the margins, while the centre endures, and the waning phantom will present a rounded appearance.

Pink casting its light upon a face will improve the incarnation. As the cheeks slope downward, a pink neck-ribbon, e. g. of a bonnet, will answer the purpose. Let it not be too large, or too intense, or else the rest of the face will suffer by the contrast. Contrast is produced by the repeated superincumbency of the phantom or complement of the other. Green trimmings in the upper part of a small bonnet will cause a green reflection on the receding and shadowy parts on the head, hence produce a heightened pink in the front and lower part, by contrast. Green reflected on the incarnate skin will make it look whiter and marble-like: green may be reflected on the forehead, pink on the cheeks, from below. Bathing in the blue waters of a Swiss lake makes the skin appear of a shining white, by absorption of the yellow rays in incarnation. A change of color may be produced either by addition of colored light, — by reflection of real, or by addition of subjective contrast-colors, — or by subtraction of certain rays by a colored screen.

Dwelling with the eye on surfaces, the effects and counter-effects are strongest in the centres; hanging on along margins, they heighten each other up to greater brilliancy, to the detriment of the centres, that appear obscured. The brilliancy and soulfulness of colors is chiefly the result of those magic hues dwelling within the vitality of the organ itself, in the living contemplation. Hence, in painting, the great object must be to direct the eye so as to cause the desired effect. The masterpieces of the master-painter, Raphael, have always been admired for these effects, aside from their other merits. If these effects were physiologically as well understood as they are intuitively appreciated, we should probably not see such very imperfect copies in either color or engraving. In his own portrait, in the Louvre, the extreme indistinctness of outlines is remarkable. It is like gazing at a face with two eyes and dilated pupil, without coaptating or accommodating the look of pathos and feeling; and the features become impressed only from dwelling on the whole at once, as it were joining regions to regions, and not lines to lines: thus the effects of color become those of heightened centres, instead of margins. No better study of this can be afforded than in his Madonna di Sisto, where the vision of the Virgin with the child is, by this delineation in tints and shades, put in a most effective contrast with the figures of reality at either side, Sixtus and St. Barbara. The beauty of the latter is comparatively soon conceived, as it were, by being drawn in distinct outlines, — the easy way-guides, — and ever exchanged for the absorbing contemplation of the visionary figures that " fetter " the gaze, no less by the diffusedness of design that causes the eager eye to infuse its own floating colors from part to part, from tint to tint, than by the sublimity of the conception. A similar effect of mellowness, to " absorb," without however affording that brilliancy of color, is perceptible in several Murillos. The subjective colors may be said to be waking dreams of the eye, — hence " entrancing."

It seems that the phantoms appear more perceptibly if projected on a background out of focus, and thus become more active at each change of focus. If from an illuminated part the eye passes over a depth in the perspective, into which it does not enter, as in the recesses of a bouquet, or the like, the phantom of the last impression will hover over that extra-focal background, not only showing that extra-focality to exist, but also by contrast heightening the impression last received. Lively contrasts are indicative of proximity. Thus the hovering obtuse counter-blot serves to indicate perspective by a double agency: by relieving the foreground, and by indicating a change of depth; besides this, they serve artificially to correct the material imperfection of color in the material. These effects are largely used in painting, although quite empirically, in order to produce perspective against the consciousness of the flatness of canvas, continually realized by the coaptation of the triangulating eye-axes, and, like all such auxiliaries to artificial perspective, require to be somewhat exaggerated, or set down more forcibly than commonly found in nature. As these effects are entirely dependent on the succession, duration, and alternation of parts, they are expressive of the whole mode of contemplation of the artist, who, after having finished the details, will stand aside, obliquely scanning the whole, and, impressed with the effects as he subjectively perceives them, will set these *finishing-touches*, teaching the eye where to roam, where to rest, what parts to take as masses, what in detail, what *order* to follow, and where to centre. They are the expression of the genius, the power, and the whole individuality of the inventor. His work *speaks* to you, as you make anything in nature speak to you after your subjective fashion. The copyist and the feeble put no such touches, if not themselves impregnated with the same spirit. A natural flower speaks to you as a unit, as an entity. The most exact copy by a fair hand will remain a delicate and exact copy of what she has distinguished, but too frequently lacks the power of individuality, while a strongly characterized representation needs no very detailed execution. For the ultimate end of art is the psychic impression.

We would here speak of the effects of oblique vision, or "sheep's eyes" (German *schief*, = oblique). It is known how works in colors and extensive prospects are scanned "*askance*," i. e. with the eye-axes strongly deflected from the forward direction. It is chiefly done by inclining the head sideward, and not facing the object directly. A very remarkable *luminosity and softness* (fusion) *of colors* is the immediate effect, while the details of form are also fused, hence less individualized and distinct. It has been frequently noted as taking place when looking through the spread legs. I have found it to take place invariably at *every deflection of the optic axis.* Looking through the legs toward the sky is deflecting the axis towards the infra-orbital margin. One-eyed persons will look at things and persons the same way, — it seems to increase the perception of light. Turning the eyes (not the head) upwards, produces the same effect, in each case not momentary, but enduring with the position, and in direct proportion more intense the greater the deflection: It can easily be tested by facing a sunset, with *the eyes fixed on it*, turning the head or the whole body around the vertical axis. The more you turn, the higher is the flush, that alternately fades down as you come to face that part directly forward. It also takes place in exercising a pressure on the front of the eyeball, and also when the pupil is dilated, as on suddenly awaking in the night, or emerging from a light room into darkness. Awaking, the eye is most sensitive, and the pencil of light admitted very great. Strong light also facilitates *sneezing*. Man and beast, if in distress for a sneeze, will squint as obliquely as possible at the strongest luminary available. By looking askance, a greater mass of light seems to become available on a sensitive part.

The stronger and more uniform the impression of a color, the less distinct the outlines, the stronger are the mutual effects of contrast. The effect on the landscape is really wonderful in an Alpine country, where on an eminence one overlooks all the landscapes and distances contained within say fifty miles, especially near sunset, where the coloredness of the light, and the diversity of its colors, add to the individualization of each part of the prospect by its proper illumination and atmospheric conditions. The orange glow of the setting sun will produce a " humid " twilight over the shadowy lake, and a " deep entrancing blue " on the craggy *silhouette* of the chain behind which it sinks, and on the bluish spruce-tree, through whose chinks it beams in all the splendor of one's own beams and splinters in the eye (lens). The haze of each separate distance will appear greenish, brownish, purplish, by a common fusion of its lights in the soft haze of its atmosphere, and by the aberration and fusion of rays within the deflected eye, the small objects being absorbed. No adaptation taking place, the whole prospect looks " as if painted," as if breathed into the sky; also from a gentle feeling of vertigo resulting from the exertion of the eyes to keep the horizontal line.

What may be the cause of these phenomena? — dispensing awhile with "effect of imagination," "attention," and the like, as we have to do with sensuous phenomena, and not with thoughts and feelings, their centre and destination.

What is the common characteristic of the phenomena? Intensity of luminosity, and deficiency in ocular touch, form. The former must have its cause in a heightened light-effect, the latter in a change of form. Light-effects are heightened by an increased pencil of light, by increased contrast, and by greater nervous sensitiveness. The latter we must reserve for the last, being the least sensibly objective, the most psychic relation of sensation, and we must commence with the physical. The consideration of indistinctness of form is still more physical among the effects. But the most physical is the first physical cause. This consists in a change of positions. In dilatation of the pupil in the position of the iris. It admits more light, hence greater intensity in the focus, and, as to form, a greater dispersion and fusion, a part of it passing through lateral parts of the lens. In the other cases, the common characteristic is a change in *pressure*. Pressure itself (uniformly exercised) has no such effect, as in accommodation for proximity the luminosity is not greater, nor vision less defined. Whatever distances are out of focus appear with a fusion of color, without any particular heightening, as is naturally to be expected from the diffusion of rays, but on parts of the retina less sensitive, as lateral objects seem to appear less defined in form and color. The ball being elastic, pressure changes its form more or less. The eye turned aside, the muscle on that side exercises probably no compression at all (at most by its own increased thickness), its sinew being no more spanned over the convexity of the eyeball, while on the opposite side the pressure must be somewhat increased by the tension of the muscle, and its application on a greater arc. The result must be a slight distortion, in the rear, towards the contracted muscle. The transverse eye-muscles, being inserted tangentially, will have but little influence by their effort to preserve the horizontal line. Such a distortion will cause the optic centre of the retina to obtain *an oblique position*, and perhaps even the optic axis will turn a little aside of it. The optic axis being obliquely incident, only one point can be in focus, all the rest extra-focal, intercepting the pencils more or less before or behind the focus. This will produce a *fusion of rays and forms.* The central intensity will sweep a greater number of elements, although each with a smaller pencil, and this may be a cause of the greater *intensity* of light, our given experience. Also the reflecting coating of the cylindric bodies, the minute conical mirrors, are struck laterally and one-sidedly instead of in their axes, and perhaps this might afford an explanation. The exact construction and the locality of sensation being as yet unknown, all being parts of the retina, this very fact of increased luminosity may lead to the determination of the sensitive locality, which must be sought at the point where in oblique incidence *more* light is collected, or where the *sums* of light-effects are increased *by obliquity.*

In reading, hence in typography, the dazzling or confusing effects of intersection by phantoms ought to be considered. *Parallel dull* ground-lines, of about equal *bulk* with the white, and somewhat *knobbed* at the end, are most suitable. All *oblique* lines, as in v, w, x, as well as *curved* and *tapering* lines, as in bowed letters, all *recurved* lines, and all fine bristles, ought to be avoided. The distinctive character of all letters ought to be at the same level, but easily to be recognized by means of their more bulky dependencies. Old English, and the small letters of German type, according to experience, are least affecting to irritable eyes, and eyes generally.

C. PRACTICAL SCIENCE.

ENGINEERING.

1. On the Use of Salt-Marsh Sods for facing the Steep Slopes of Parapets, Terraces, etc. By Lieutenant E. B. Hunt, U. S. Corps of Engineers.

Along our Northern seaboard, and especially on our New England coast, there are numerous and extensive salt marshes, which are overgrown with a very thick turf of marsh grass. They are usually alternately overflowed and left dry by the rising and falling tide. The grass with which they are covered is, so far as I have observed, chiefly of three varieties. The coarsest or sedgy grass is found in those parts which are most deeply covered by the tide-water, and is much less thickly set on the surface than the others. I am inclined to think that this may be but the eel-grass of deeper water, slightly changed by rising out of the water. The second variety is that commonly known as "fox marsh" grass, and is quite fine and thick, of a lightish green tint, and uniform in character. The third variety, known as "black marsh" grass, grows on land which is wet, but not usually flooded, and is of a much darker and richer shade of green than the others. Fox and black marsh hay is much used for packing ice, &c., for which it commands quite a remunerating price. The effect of draining is said to be, that the coarse grass is replaced by fox marsh, and this in turn by black marsh. The fox and black marsh are said to require rains for their healthy growth, and to be affected by droughts even when rooted in salt water.

If the turf of either of these three varieties be cut into, it will be found to consist almost entirely of a compact mass of fine roots, the roots of the coarse grass being larger and less tenacious. The color of the turf grows darker in descending, and the toughness, which is a maximum on the surface, decreases with the depth, until, at the depth of about two feet, the fibrous structure loses its predominance, and the material becomes mucky and deficient in strength. Below this it is a true peat, with more or less impurities, according to its locality. The section is what might be anticipated from knowing that this peat is a gradual result from the decay of the lower roots, and that a surface growth of vegetation is the origin of the entire bed. The sod itself has a great degree of strength, elasticity, and durability. It is like a sponge in its absorption, and in its recovery of shape when compressed, if the roots are not broken. Taken on to dry land, it loses a large part of its weight, from the drying out of its water. In good localities, sods can, without difficulty, be got out as much as eighteen inches deep, and strong enough to bear pretty rough handling without injury. The roots are partly macerated at the first, but drying removes this, and adds greatly to their strength.

With the sanction of the Chief Engineer, I have during this season applied these sods for facing the breast-height slope of over 1,000 feet of battery crest at Fort Adams, Newport. Parapet breast-heights, or interior slopes, are generally built with masonry walls to within eighteen inches of the crest, and the remainder consists of a slope which it is desirable to have as steep as one base to three vertical. It has proved impossible to make any common grass sodding which would stand on these slopes, nor has any satisfactory construction yet been found for this case, unless it be by the use of salt-marsh sods. After careful observation, there seems to be every reason to hope that they will perfectly meet the demands of this construction. On Fort Adams alone there is an extent of over two miles of such crest, whence its importance is apparent.

These sods were used for building a parapet of a fort at Gloucester, Mass., during the war of 1812, and General Totten thus employed them in various instances at that time. On seeing the Gloucester parapet, in 1850, I was struck with its excellent preservation, and indeed I think it has lasted decidedly better than rough masonry would have done in its place. There is one great difference between this construction and that of Fort Adams. At Gloucester the sods are cut thin, and laid with their edges to the face; at Fort Adams the sods are from twelve to fifteen inches thick, and laid on their edges, with the grass facing outwards. I have been told that in Ireland field walls for fences are made by cutting these sods in small blocks, and laying them up, grass outwards, on each face, filling between the roots with bits of sod, and covering the whole with loam. The sods are cut from trenches along the wall, and the top of the wall is planted with vegetables. It is found that the grass continues to grow on the sides, and the walls themselves are said to be very durable. The poor cottages with peat walls which prevail in some parts of Europe also illustrate this durability. The well-known antiseptic property of peat is evidently closely related to this durability of salt-marsh sods.

The mode in which work has proceeded in facing the Fort Adams breast-heights is this. The fox marsh sods are cut with a long hay-knife, by plank guides, into blocks eighteen inches square, and from twelve to fifteen inches deep. They are moved by hand-barrows, and are boated to the work. The breast-height wall, being cleared and its top plastered and asphalted with a heavy brush coat, is furnished with slope guides of six inches base to eighteen inches vertical. The precise angle to which each sod should be cut is measured by a bevel square, and applied on a side of the sod, which is then pared to the required bed by means of a long two-handled knife. It can be cut with great neatness, and, by successive trying and trimming, the joints are made nearly perfect for the entire depth. The sod is placed on its edge, and slightly projecting over the wall, as a roof to its exposed angle. It is then compressed by means of a lever and plank, so as to thoroughly close the joints, and to prevent any opening from drying. Having laid a line of sods in this manner, they are backed with earth, and covered with common sods, which by guttering the back, are doubled over to form a little over an inch of the face slope. Thus only grass is seen. It is already apparent that the marsh grass will continue to grow, and that the fox grass will become black grass. The construction has very great solidity, and can hardly fail to stand unimpaired for a long time. Its military qualities as a parapet cap, and for embrasure cheeks, are admirable, as it is free from stones, and cannot scatter fragments when struck by a ball.

As the availability of these sods for various constructions seems not to be much known, I have thought it might be of some general use to lay before the Association this notice of a strictly professional construction. It has seemed to me unquestionable that these sods could be in many instances advantageously used for facing *terrace slopes*, where the height is not great, and when a lack of space makes steep slopes desirable. Thousands of New England cottages which have or need terrace slopes are within striking distance of marsh sods. By laying two or three depths of sods on masonry principles, quite high facings could be made, with slopes of one base to three vertical, on which a rich coating of black marsh grass would almost surely grow, if the sods were cut in the spring; or, if desired, the sods could be laid with the edges outward, making a brownish, regular wall. This application is one which I have never seen or heard of, but I am so confident of its advantages in many cases, that I do not hesitate to recommend its trial.

The species of sod-fence before described must, I imagine, be of great value in some localities where timber is costly, and it seems deserving of the attention of those interested. There is another application, which I have never heard of or seen, but which seems to me very promising. This is in the construction of blind drains. The walls, bottom, and top of a drain could, with much facility, be built of such marsh sods. The sod edges would form good drain walls, and they would doubtless suffice for some open drains. They would present the great advantage over stone of giving free passage for water through their masses. They could readily be cut so as to be used for stairs in terrace slopes, and elsewhere. They would also form a durable bordering for walks. By laying them grass downwards, and smoothing the root faces, they would make a footpath or sidewalk of great softness, elasticity, dryness, and permanence. A dike over a marsh at Newport, built of sods cut along its base, has stood in good order as a footway for some five years. They have also been used for filling in wooden wharves at Newport, where they stand the wash perfectly. There are, doubtless, many other such uses, which ingenuity would soon discover, should attention be thus directed. When we remember that India-rubber was for many years only known as a means of erasing pencil-marks, and that gutta-percha was, until very recently, only used for axe-handles, we shall be inclined to study new applications of old materials in a very hopeful spirit.

The following papers were presented, and most of them were read, but no copy of them has been furnished for publication: —

I. MATHEMATICS AND PHYSICS.

1. On the Mean Distance from the Sun, Inclination of Orbit, and Equatorial Character of the Asteroid Planet. By Professor Stephen Alexander.
2. On the Physical Phenomena presented during the Solar Eclipse of May 26, 1854. By Professor Stephen Alexander.
3. Some Additions to the New Method of Astronomical Observations in R. Ascension and in Declination. By Professor O. M. Mitchel.
4. On Binocular Vision. By Professor W. B. Rogers.
5. On the Winds. By Captain Charles Wilkes.
6. On the Zodiacal Light. By Rev. George Jones.
7. On the Colored Projections from the Edge of the Sun, as observed during Solar Eclipses. By Professor Joseph Henry.
8. The Effect of the Gulf Stream on the Temperature of the Atlantic Coast. By Dr. James Wynne.

II. CHEMISTRY, NATURAL HISTORY, AND GEOLOGY.

9. On the Method of analyzing the Sulphate, Arsenate, and Molybdate of Lead. By Professor J. Lawrence Smith.
10. On some Arrangements to facilitate Chemical Manipulations. By Professor R. E. Rogers.
11. Notice of Remarkable Specimens of Crystallized and Arborescent Gold, from California. By W. P. Blake.
12. On the Deposits of Fossil Microscopic Organisms at Monterey, California, with Specimens. By W. P. Blake.
13. Notes upon the Geology and Mineral Association of the Cinnabar Sulphuret of Mercury, of New Almaden, Cal., with Specimens. By W. P. Blake.
14. On the Stratagraphical Position of the Coal-bearing Rocks below the Upper Red Shale and Carboniferous Limestone of the Middle and Southern States. By Professor William B. Rogers.

15. On the Configuration of the Soil of New England. By Professor A. Guyot.

16. On the Occurrence of Proboscidon Remains in Wisconsin. By Edward Daniels.

17. On the Character of the Lead Deposits of the Upper Mississippi. By Edward Daniels.

18. On the Occurrence of Silicious Grits as Veinstones in the Lead Mines of Wisconsin. By Edward Daniels.

19. Some Observations on the Northern Outcrop of the Illinois Coal Formation. By Edward Daniels.

20. Contributions to our Knowledge of the Geology of Nebraska and the *Mauvaises Terres.* By Professor James Hall.

21. Notes upon the Genus Graptolithes. By Professor James Hall.

22. On the Development of the Septa in the Genus Baculites, from the extreme Young to the Adult State. By Professor James Hall.

23. On some Effects produced by the Trap on the adjoining Liasic Rocks of Virginia. By Professor W. B. Rogers.

24. Notes on the Geology of Western India. By Rev. Ebenezer Burgess.

25. Remarks on the Geological Formation of Table Mountain, Cape of Good Hope. By Rev. Ebenezer Burgess.

26. On the Metamorphic Rocks of Nahant, Mass. By Professor Louis Agassiz.

27. On Gradation among Polypi. By Professor Louis Agassiz.

28. On the System of Zoölogy. By Professor Louis Agassiz.

29. Notes on the Nature of the Coverings of the Seeds of Magnolia, and on the Diœcious Character of Species of Plantago. By Professor A. Gray.

30. Motions effected by Plants result from the Contractions of Cells. By Professor A. Gray.

31. On the Probability of a Decline in the Production of Gold. By J. D. Whitney.

32. On the Big-root of California, a new Genus of Cucurbitaceæ (Megarrhiza). By Professor John Torrey.

EXECUTIVE PROCEEDINGS

OF THE

PROVIDENCE MEETING, 1855.

HISTORY OF THE MEETING.

THE Ninth Meeting of the American Association for the Advancement of Science was held at Providence, R. I., commencing on Wednesday, August 15, and continuing through Wednesday, August 22.

The number of names registered in the book of members in attendance on this meeting was one hundred and sixty-six. Seventy-four new members were chosen, of whom fifty-eight have since accepted their appointment. Sixteen others have joined the Association by virtue of Rule 2 or 3. Four others paid, without signing the constitution. Ninety-three papers were presented, most of which were read, but only a part have been printed. Some were thought unworthy of publication, and, in other cases, copies have not been furnished by their authors.

The Meetings of the Association were held in the Halls of Brown University.

The Annual Address was delivered by the retiring President, Professor JAMES D. DANA, on Friday evening, August 17.

A Report on the Recent Progress of Organic Chemistry was read by Dr. WOLCOTT GIBBS, on Saturday morning, August 18.

No lengthened abstract of the proceedings, scientific and executive, of the Providence Meeting of the Association is necessary in this place, as they are contained in full in the papers and resolutions printed in this volume. The revisal of the constitution, and the codification of the various scattered resolutions, of a permanent character, passed since the organization of the Association, excited a brief debate, when the subject was postponed for another year.

The officers elected for the next meeting are Professor James Hall, of Albany, President; Dr. B. A. Gould, of Cambridge, General Secretary; and Dr. A. L. Elwyn, of Philadelphia, Treasurer. The Permanent Secretary, Professor Joseph Lovering, of Cambridge, retains his office for three years from August, 1854.

The Association voted to hold their next meeting at Albany, N. Y., on Wednesday, the 20th of August, 1856, having received an invitation to visit that city from several prominent citizens.

During the meeting at Providence, the members of the Association, and their ladies, were elegantly entertained on different evenings as follows: — By President Wayland, on Wednesday, August 15th; by Z. Allen, Esq., and by Gen. E. Dyer, on Thursday, the 16th; by John C. Brown, Esq., on Friday, the 17th; by James Y. Smith, Esq., Mayor of the City, on Monday, the 20th; and by Dr. S. B. Tobey, on Tuesday, the 21st.

On the afternoon of Wednesday, August 22, the members of the Association, and their ladies, were entertained at dinner by invitation of the Local Committee, and through the hospitality of the citizens of Providence. The company assembled in University Hall, and at 2 o'clock, P. M. proceeded thence to the table, which was spread under a beautiful tent upon the College green. After partaking of the bountiful repast, the members of the Association were briefly welcomed by Professor A. Caswell, in behalf of the Local Committee, and the several votes of thanks, hereafter printed, were read, and, after appropriate remarks from members of the Association and citizens of Providence, were passed by acclammation. The members of the Association were compelled, by want of time, to decline an invitation to join in an excursion down the bay to Bristol Ferry.

Arrangements were also made by the Local Committee, which gave the members of the Association access to the following places: — University Library, Providence Athenæum, Butler Hospital, Swan Point Cemetery, City Reform School, State Prison, P. Allen & Sons' Print-Works (North End), Eagle Screw Factory, 21 Stevens Street (North End), Corliss and Nightingale's Steam-Engine Manufactory (North End), Providence Forge and Nut Company's Works (North

End), Gorham and Company's Silver Ware Manufactory, 12 Steeple Street, Providence Gas Works, Benefit Street (Lower End), Rolling Mill, Benefit Street (Lower End), Sackett, Davis, and Potter's Jewelry Manufactory, corner of Richmond and Friendship Streets, Providence Machine Company's Works, Eddy Street, New England Screw Factory, Eddy Street, Atlantic DeLaine Company's Mills, at Olneyville (now manufacturing cottons and cassimeres), and J. Dunnell and Company's Print-Works, Pawtucket.

RESOLUTIONS ADOPTED.

Resolved, That this Association regards the preparation of an Index of Papers on subjects of Mathematical and Physical Science, proposed in Lieutenant Hunt's communication to the Section of Mathematics and Physics, as one of the most important and valuable enterprises for advancing science which can now be undertaken, and that it would invite the co-operation of such persons as are able effectively to labor for this end.

Resolved, That the Committee on Standard Weights, Measures, and Coinage be authorized to communicate with other associations, or public bodies, or with individuals, in regard to the establishment of universal and permanent uniformity in weights, measures, and coinage; and be requested to report at the next meeting.

Resolved, That the Committee on Weights and Measures be instructed to correspond with Great Britain and France, and such other countries as may seem desirable, on the subject of Coinage; and to present a memorial to Congress, at its next session, in favor of adopting the decimal system of weights and measures.

Resolved, That the draught of the Constitution, proposed by the Committee appointed to revise the same, be laid over till the Albany meeting.

Resolved, That Professor A. D. Bache and Professor Joseph Lovering be a Committee to report to the Standing Committee in regard to the continuance or discontinuance of old Special Committees.

Resolved, That the following Committees be discharged: —

1. Committee to Memorialize the Legislature of Ohio on the Subject of a Geological Exploration of that State.

2. Committee to Memorialize Congress in Relation to a Geographical Department of the Congress Library.

3. Committee to Memorialize Congress for an Appropriation to enable Professor O. M. Mitchel to perfect and apply his new Astronomical Apparatus.

4. Committee to take Proper Measures in Regard to the Annular Eclipse of May 26th, 1854.

Resolved, That the Standing Committee return their best thanks to the Permanent Secretary, for the very thorough manner in which he has executed the duties of his office; and congratulate the Association on the success of the able and energetic measures pursued by him.

Resolved, — 1. That the papers read at this meeting be referred to the Standing Committee, to determine in reference to their publication.

2. That the papers not accepted for publication be returned to their authors.

Resolved, That the Permanent Secretary be allowed to put the Proceedings of the Providence Meeting to press one month after the adjournment of the Association, and that 1,500 copies be printed.

Resolved, That the Permanent Secretary be directed to cause 300 extra copies of the Address of the retiring President to be struck off, and placed at the disposal of the author.

Resolved, That the Permanent Secretary be directed to cause 300 extra copies of the Report of Dr. Wolcott Gibbs to be struck off, and placed at the disposal of the author.

Resolved, That the Standing Committee extend an invitation to foreign learned societies, and individuals devoted to science, to attend the annual meetings of the Association.

Resolved, That the next meeting of the Association be held at Albany, commencing on the third Wednesday of August [20], 1856.

Resolved, That the Standing Committee of the Association meet at the Delavan House in Albany, on Tuesday evening, August 19, at 9 o'clock, P. M., in order to make arrangements in advance to facilitate the business of the meeting.

Resolved, That the Local Committee for the Albany Meeting be requested to secure the services of Mr. PARKHURST, and a proper corps of assistants, if required, to give short-hand reports of the discussions at the meeting. In case Mr. PARKHURST cannot be obtained, that then the Local Committee shall be authorized to secure the services of the best phonographer to be found.

Resolved, That the Standing Committee be authorized to act for the Association in any matters of business which may not have been completed at the time of the adjournment of the Providence Meeting.

RESOLUTIONS IN HONOR OF THE LATE HON. ABBOTT LAWRENCE OF BOSTON. Presented by PROFESSOR A. D. BACHE.

I RISE to make an announcement, and to offer a resolution of condolence in reference to the decease of one of the most munificent patrons of science in the United States, — the Hon. Abbott Lawrence, — whose career of beneficence has just been closed by death.

After struggling for eighteen months against disease, Mr. Lawrence died on Saturday last, at his residence in Boston, at the age of between sixty-two and sixty-three years.

Having in middle life acquired by his own exertions an ample fortune, Mr. Lawrence devoted his large means to the good works of charity, beneficence, and hospitality, and to the fostering of science, learning, and art.

It is chiefly as the founder of the Lawrence Scientific School of Harvard that I wish to dwell upon his claims to your consideration. At the outset he endowed that institution with the sum of fifty thousand dollars, and there is confidence that his will contains provision for giving still further development to this great work.

The light in which this endowment was regarded by the family of Mr. Lawrence, and which shows how nobly they are associated in his great and good deeds, is revealed by a letter from his brother Amos, which I take the opportunity to read to the Association.

"Wednesday Morning, June 9, 1847.

"DEAR BROTHER ABBOTT: — I hardly dare trust myself to speak what I feel, and therefore write a word to say, that I thank God I am spared to this day, to see accomplished by one so near and dear to me this last, best work ever done by one of our name, which will prove a better title to true nobility than any from the potentates of the world. It is more honorable and more to be coveted than the highest public station in our country, purchased as these stations often are by timeserving. It is to impress on unborn millions the great truth, that our talents are trusts committed to us for use, and to be accounted for when the Master calls. This magnificent plan is the great thing you will see carried out, if your life is spared; and you may well cherish it as the thing nearest your heart. It enriches your descendants in a way that mere money never can do, and it is a better investment than any one you have ever made.

"Your affectionate brother,

"Amos.

"To Abbott Lawrence."

The success of that institution has been already very great, and within a few days of his decease Mr. Lawrence had the high gratification to receive the assurances of this from one of our colleagues best able to appreciate the results (Professor Peirce), who, having attended the annual examinations of the school, found such evidences of successful study, and ample and sound instruction, that he felt constrained to express to the dying patron of the School his convictions of the entire success of his judicious plans.

It will be recollected by some of the members of the Association, that Mr. Lawrence was selected by President Taylor as his Secretary of the Navy. This offer, however, he felt compelled to decline.

On visiting Washington, he told me that one regret which he had in declining the post was, that he should not have the opportunity to organize the Nautical Almanac, for which an appropriation had been made by Congress, but that he would recommend earnestly to his successor the plan which since has been so well executed.

As Minister to the Court of St. James, Mr. Lawrence had many opportunities of showing to scientific men his high appreciation of the career to which they were devoted. Those who visited Europe always found him ready to aid them in accomplishing the objects of their journey, and those who desired communication with Europeans were assisted by all the means in his power.

The science of the United States owes a debt of deep gratitude to Mr. Lawrence, which the resolutions I now present but feebly shadow forth.

Resolved, That the American Association for the Advancement of Science have heard with deep regret of the decease of the Hon. Abbott Lawrence, who, by the munificent foundation of the Lawrence Scientific School of Harvard University, has identified his name with the progress of science in the United States.

Resolved, That the members of the Association offer their sincere condolence to the bereaved family of Mr. Lawrence.

Resolved, That the President and Secretaries of the Association communicate these resolutions to the family of the deceased.

These resolutions, signed by the President and Secretaries of the Association, were transmitted to Mrs. Lawrence, with the following note from the Permanent Secretary: —

"Cambridge, September 11, 1855.

"While I have the honor officially to transmit to you the resolutions passed by the American Association for the Advancement of Science, in acknowledgment of the great public and private loss occasioned by the death of your late husband, the Hon. Abbott Lawrence, may I be permitted also to express to you my individual sympathy with you in your severe affliction.

"Very truly yours,

"JOSEPH LOVERING,

Permanent Secretary.

"MRS. ABBOTT LAWRENCE."

CORRESPONDENCE.

Providence, July 20, 1855.

For several years extensive experiments have been made in France, upon a large scale, with the view of ascertaining how far the rivers of that country could be replenished with fishes, which of late had become very scarce. The experiments have been entirely successful. With such results before us, and when we hear daily how our rivers are losing their fishes, it would seem desirable that the Association should recommend, or at least countenance, the measures which are likely to be proposed in the State of New York, with reference to fisheries and artificial fecundation of fishes.

The motives for such exertions are the salubrity of that article of food, the usefulness of certain regulations to secure its production, and the advantage the practices employed in artificial fecundation afford to scientific investigations.

Scientific men, above all others, should be anxious to secure to the community an abundance of fish, when it is ascertained that no other article of food supplies so promptly, and so effectually, the waste of the brain arising from mental exertions.

Wherever artificial fecundation has been practised, fishes have become abundant, even where they had been scarce before. For more than one hundred years the breeding of fishes has been successfully practised in Germany, especially that of carps.

Finally, it is by artificial fecundation embryologists have recently obtained the most favorable opportunities of tracing their investigations.

Taking into consideration all these circumstances, it seems to me desirable that the Standing Committee offer a resolution to the Association, to the effect of approving the measure which the Legislature of New York may pass to promote pisciculture in that State.

L. AGASSIZ.

TO THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

The Committee to whom the subject of the introduction of establishments for fish-breeding in this country was referred, beg leave to recommend, —

That a Committee be appointed to memorialize the Legislature of New York, with reference to the promotion of fish-breeding in that State.

JAMES D. DANA.

REPORTS.

1. Report of Professor S. F. Baird, late Permanent Secretary of the American Association for the Advancement of Science, on the Distribution and Disposal of the Volumes of Proceedings.

The objects of the report will perhaps be best answered by taking up the volumes in succession, and giving a brief account of the circumstances attending their publication.

First Meeting, Philadelphia, 1848.—This volume was edited and published in Philadelphia, and distributed from that point to members. The extent of the edition is not known, but, as in the case of the Cambridge volume, it is understood that nearly all the members on the list received copies, irrespective of their having paid their annual fees. No statement of the distribution of this volume, as of the Cambridge and Charleston ones, having been made by the Secretary preceding the undersigned, he can only give the number of 286 copies as received by him from various sources. The number delivered to Professor Lovering amounts to 86 copies.

Second Meeting, Cambridge, 1849.—Of this volume, 700 copies were published by the Association, all of which were expended by issues to members, distribution to learned societies abroad, &c., no particulars of which are on record. The undersigned received no copies whatever from the previous Secretary of the Association. He however purchased from Munroe & Co., of Boston, 100 copies, at $1.00 each, with a few from other parties at $1.50; and collected between forty and fifty volumes from the Smithsonian Institution, Professor Henry, and others, who liberally gave back the copies they had received in return for extra subscriptions. Delivered to Professor Lovering 32 copies.

Third Meeting, Charleston, 1850.—This volume was published by the liberality of the city of Charleston, without expense to the Association, and copies given to all members who had paid their dues. From Mr. Herrick were received 184 copies, and others from Dr. Ravenel of Charleston, of which no account was sent, and the number cannot now be exactly ascertained, but is believed to have been about 300. Delivered to Professor Lovering 292 copies.

Fourth Meeting, New Haven, 1850.—Of this, the first volume published by the undersigned, 1,000 copies were issued; of these, 30 copies were lost on the steamboat on which they had been shipped for transmission to the Albany meeting. Delivered to Professor Lovering 274 copies.

Fifth Meeting, Cincinnati, 1851.—This volume was published by the citizens of Cincinnati, in an edition of 999 copies, of which 60 were retained by the Cincinnati Committee for subscribers to the publishing fund, 55 for other members of the Association, and 182 for the purpose of making up by their sale a balance due on the volume after the subscriptions had been collected. The remaining 702 copies were sent to the undersigned. Delivered to Professor Lovering 494 copies.

Sixth Meeting, Albany, 1851.—This volume was also published by subscription among the citizens of Albany, and 1,000 copies placed at the disposal of the Association. Of these, 374 copies were delivered immediately on their publication to members who had paid their dues; much the largest number ever issued in this way at any one time. Delivered to Professor Lovering 353 copies.

It is a difficult matter to get at the exact number of copies of the Proceedings issued to members, as no strict account of these was kept. The earlier volumes were distributed to nearly all the names recorded on the list, whether the parties had paid their annual dues or not. As this was found to encourage remissness in paying the annual assessments, the Association directed that the New Haven and succeeding volumes should be issued only to those who had paid their dues, providing also for the elimination, after due notice, of the names of delinquents. In this way a great saving was effected in the stock of publications, which thenceforward became a source of profit to the Association, instead of requiring the extra aid of a few liberal members to meet the expenses over and above the amount in the hands of the Treasurer.

The number of copies thus distributed for the two regular volumes (New Haven and Albany), after this order, has varied from three to four hundred, exclusive of sales of back volumes to new members, or to non-members, either directly, or through agents. Lists have been furnished from time to time by the Treasurer, of persons paying their dues, and consequently entitled to the corresponding volumes, or certificates furnished by him to parties have been presented and received as vouchers of the same right. No minute record of the volumes so issued has been kept, as the stock on hand by actual count, added to the number sold, or distributed to foreign and domestic institutions (of which the accounts are carefully preserved), always furnishes the data for ascertaining the volumes thus expended.

One not inconsiderable expenditure of volumes has been in the number lost at meetings, by being carried away surreptitiously or accidentally from the office of the Association. It has been impossible to keep these books always in a safe place, or to exercise that constant supervision over them which would prevent this loss. In a single meeting, as many as twenty or thirty copies have gone in this way, as nearly as could be ascertained; taken, not by members in all probability, but by others, some of whom doubtless supposed these volumes intended for gratuitous distribution.

In addition to the copies of the volumes distributed to members and institutions, a considerable number has been sold by booksellers or agents, as mentioned in the accompanying statement, in the aggregate amounting to 693 volumes. The gross receipts by the Treasurer for these volumes have amounted to over 1,000 dollars, constituting no inconsiderable source of revenue, and, at the same time, putting it in the power of institutions and individuals, at home and abroad, not members, to procure the records of the Association.

Respectfully submitted,

SPENCER F. BAIRD.

APPENDIX A.

S. F. BAIRD in Account with Volumes of the Proceedings of the American Association.

Dr.		I. *Philadelphia.*	Cr.	
Rec'd from W. P. Hazard,	172	Sold, . . .	105	
" " E. C. Herrick,	114	In hands of Agents,	25	
	——	European Distribution,	27	
	286	Issued to Members, say	43	
		Sent Professor Lovering,	86	
			——	286

II. *Cambridge.*

Purchased of Munroe, .	100	Sold, ...	60	
Presented by Smithsonian Institution, and obtained in other ways, . .	50	With Agents, . .	25	
		European Distribution,	27	
		To Members, say .	6	
		To Professor Lovering,	32	
	150			150

III. *Charleston.*

From E. C. Herrick, .	184	Sold, ...	123	
From Ravenel, ...	?	With Agents, .	25	
		European Distribution,	27	
		To Members, . .	?	
		To Professor Lovering,	292	
				467

IV. *New Haven.*

From Printer, . .	1,000	Sold, ...	278	
		With Agents, .	68	
		Lost by steamboat, .	30	
		To Europ. Institutions,	3	
		To Amer. "	8	
		To Members, say .	339	
		To Professor Lovering,	274	
				1,000

V. *Cincinnati.*

Edition published, . .	999	Sold, ...	72	
		With Agents, .	25	
		To Europ. Institutions,	27	
		To Amer. "	8	
		To Members, say .	131	
		To Subscribers of $ 5,	60	
		Kept by Cincin. Com.	182	
		To Professor Lovering,	494	
				999

VI. *Albany.*

Edition published, . . .	1,000	Sold, ...	55	
		With Agents, .	160	
		To Europ. Institutions,	28	
		To Amer. "	8	
		To Members, say	396	
		To Professor Lovering,	353	
				1,000

APPENDIX B.

List of European Institutions to which Copies of the Proceedings of the American Association were distributed by S. F. Baird in 1852 – 53.

	Volumes.					
	I.	II.	III.	IV.	V.	VI.
Stockholm, — Kongliga Svenska Vetenskaps Akademien,	*	*	*		*	*
Copenhagen, — Kongel. danske Vidensk. Selskab,	*	*	*		*	*
Moscow, — Soc. Imp. des Naturalistes,	*	*	*		*	*
St. Petersburg, — Acad. Imp. des Sciences,	*	*	*		*	*
" Kais. Russ. Min. Gesellsch.,	*	*	*	*	*	*
Amsterdam, — Acad. Royale des Sciences,	*	*	*		*	*
Haarlem, — Holl. Maatschappij der Wetenschappen,	*	*	*		*	*
Berlin, — K. P. Akad. der Wiss.,	*	*	*		*	*
Breslau, — K. L. C. Akad. der Naturf.,	*	*	*		*	*
Franckfurt, — Senckenbergische Gesellschaft,	*	*	*		*	*
Göttingen, — Königl. Gesellschaft der Wiss.,	*	*	*		*	*
Munich, — K. B. Akad. der Wiss.,	*	*	*		*	*
Prag, — K. Böhm. Gesellschaft der Wiss.,	*	*	*		*	*
Vienna, — K. Akad. der Wiss.,	*	*	*		*	*
Bern, — Allg. Schw. Gesellschaft,	*	*	*		*	*
Geneve, — Soc. de Physique et d'Hist. Nat.,	*	*	*		*	*
Bruxelles, — Acad. Royale des Sciences,	*	*	*		*	*
Liège, — Soc. Royale des Sciences,	*	*	*		*	*
Paris, — Institut de France,	*	*	*		*	*
Turin, — Accademia Reale delle Scienze,	*	*	*		*	*
Madrid, — Real Acad. des Ciencias,	*	*	*		*	*
Cambridge, — Camb. Philosophical Society,	*	*	*		*	*
Dublin, — Royal Irish Academy,						*
Edinburgh, — Royal Society,	*	*	*	*	*	*
London, — Board of Admiralty,	*	*	*		*	*
" East India Company,	*	*	*		*	*
" Museum of Practical Geology,	*	*	*	*	*	*
" Royal Society,	*	*	*		*	*
	27	27	27	3	27	28

Copies of the fourth volume were purchased by the Smithsonian Institution, and presented to the above Institutions.

American Institutions receiving Copies of the Proceedings of the American Association by Vote of the Association.

	Volumes IV.	V.	VI.
American Academy, *Boston*, ...	"	"	"
Natural History Society, *Boston*, ...	"	"	"
New York Lyceum, *New York*, ...	"	"	"
Philadelphia Academy of Natural Sciences, *Philadelphia*,	"	"	"
American Philosophical Society, "	"	"	"
Western Academy of Natural Sciences, *Cincinnati*, .	"	"	"
Cleveland Academy of Natural Sciences, *Cleveland*, .	"	"	"
Smithsonian Institution, *Washington*, . . .	"	"	"

Report of the Committee to examine into the Distribution of Copies of Proceedings of the American Association for the Advancement of Science, by the late Permanent Secretary, Professor S. F. Baird.

The undersigned, a Committee appointed for the purpose, have examined the report of the late Permanent Secretary of the American Association, in reference to the distribution of volumes of Proceedings in his charge, and find the account as satisfactory as the nature of such transactions will allow, and they fully accord to the late Secretary the commendation which they think he deserves for his management of the trust committed to his care.

Joseph Henry,
J. S. Hubbard.

To the Standing Committee of the American Association for the Advancement of Science: —

The undersigned, a Sub-Committee to whom was referred the documents presented by the late Permanent Secretary, Professor Baird, would report, —

1st. That these documents consist of a statement of *estimates* of the number of volumes received and distributed, but without any vouchers.

2dly. Of a list of institutions to whom the Proceedings have been sent.

3dly. Of a certificate signed by Professors Joseph Henry and J. S. Hubbard, that the accounts seem as satisfactory as the nature of the case permits, and according the commendation which they think he deserves.

The Sub-Committee do not feel themselves called upon to express any judgment in the premises.

James D. Dana,
B. A. Gould, Jr.

Providence, August 19, 1855.

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2. Report of the Committee on the Solar Eclipse of May 26, 1854.

The Committee on the Solar Eclipse of May 26, 1854, respectfully report, that they attended to the duties confided to them, distributing as widely as possible the information in regard to it, and making extended arrangements for observation. The unfavorable character of the weather on the day, in that portion of the United States in which the eclipse was annular and central, is well known to the members. The observations collected have been published in the Astronomical Journal, and the Committee now request to be discharged.

A. D. Bache, *Chairman.*

Providence, August 22, 1855.

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3. Report on Mr. Bassnett's Theory of Storms. By Professor Joseph Henry.

Professor Henry presented the following verbal report on Mr. Bassnett's Theory of Storms.

Professor Henry stated that Mr. Bassnett's theory had been tested by the Committee, by observing the weather, and noting its correspondence or want of correspondence with Mr. Bassnett's predictions. The predictions of the theory were verified during the first ten days tolerably well, but were found worthless during the remaining time of observation, or for nearly two months.

4. Report of the Committee on Dr. Brainerd's Paper.

The Committee to whom the question respecting the non-publication of Dr. Brainerd's paper was referred would report, —

First. That the paper of Dr. Brainerd was withdrawn from the list of papers for the Cleveland Volume, by the regular and authorized action of the Association.

Second. That the character of the paper was such, — its conclusions so erroneous, and its reasonings so false, — that any other action would have been wanting in fidelity to the interests of the Association and the science of the country.

James D. Dana,
Benjamin Peirce.

INVITATIONS.

To the President of the Association: —

The Local Committee invite the Association to join them in an excursion down the Bay to Bristol Ferry, on one day during the session. Tuesday or Wednesday of next week will be convenient.

Very respectfully,
A. Caswell, *Chairman.*

Saturday, August 18, 1855.

In reply to this invitation, it was resolved, —

That the Association presents its sincere thanks to the Local Committee and citizens for their kind invitation, and regrets exceedingly that the want of time will prevent the members from accepting it.

To the President of the Scientific Association: —

The Committee of Reception have the pleasure of inviting the members of the Association for the Advancement of Science, together with their ladies in attendance, to a Complimentary Dinner, on Wednesday, the 22d, at 2 P. M.

For the Committee,
A. Caswell.

Monday, August 20, 1855.

Rooms of the Providence Young Men's Christian Association,
No. 56 Broad Street, August 11th, 1855.

MEMBERS OF THE SCIENTIFIC ASSOCIATION : —

GENTLEMEN, — It becomes my pleasing duty to inform you, that at the meeting of the Board of Managers of this Association, holden on the 9th instant, it was voted, —

"That the members of the Scientific Association be invited to visit our Library and Reading-Room as often as may suit their convenience during their session in our city."

N. B. — Rooms open from 9 A. M. to 9½ P. M.

Respectfully yours,

WILLIAM C. MILLS,
Sec. of Board of Managers of the P. Y. M. C. Association.

Providence, August 17, 1855.

SIR, — The Cabinet and Library of the Rhode Island Historical Society will be open for inspection by the members of the American Scientific Association, on the afternoon of Monday, the 20th instant.

Any members of the Association who are interested in historical investigations can obtain ready admission to the rooms at any other time, by application to any member of the Society.

Very respectfully, your obedient servant,

ALBERT G. GREENE, *President R. I. Hist. Society.*

PROF. JOHN TORREY, *President Amer. Scientific Association.*

TO THE PRESIDENT OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE : —

DEAR SIR, — The Providence Franklin Society, a body especially devoted to the study of physical science, has voted to invite the members of your Association to visit its Cabinet during your session. A Committee, of which the President of the Society is chairman, is charged with the agreeable duty of communicating this invitation.

We shall be happy to meet the members of the Association, and any others who may favor us with a visit, on the afternoons of Friday and Saturday, the 17th and 18th of August, at which time some of our Committee will be present. The Cabinet will be accessible at any other time, on application to any members of the Society.

On behalf of the Committee,

CHARLES W. PARSONS,
President of the Providence Franklin Society.

Providence, Cabinet of the Society, 20 North Main Street (up stairs),
August 15, 1855.

Providence Athenæum, August 14, 1855.

DEAR SIR, — I have the pleasure of enclosing to you, to be communicated, the vote of this Institution, tendering the use of its Rooms and Library to the members of the American Association for the Advancement of Science, while in this city.

WM. G. PATTEN, *Vice-President.*

DR. WOLCOTT GIBBS, *General Secretary of the American Association for the Advancement of Science, Providence.*

Providence Athenæum, August 6, 1855.

Voted, That the members of the " American Association for the Advancement of Science " be authorized and invited to visit and use the Library of the Athenæum, and its Rooms, during the session of the Association about to be held in this city.

JOHN GORHAM, *Secretary.*

WM. G. PATTEN, *Vice-President.*

REPORT OF THE PERMANENT SECRETARY.

THIS report is made by the Secretary in anticipation of the adoption of the second clause of Article 20 of the new Constitution to be proposed, which requires a statement of " the business of which he has had charge since the last meeting of the Association." This business has consisted, — 1. of general correspondence; 2. of notification of their election to new members, and of the issue of the circular for the Providence meeting; 3. of the preparation, publication, and distribution of the Washington volume, and of large extra editions of Professor Peirce's Address, and Dr. B. A. Gould's Eulogy; and 4. of the collection of assessments, and the payment of bills.

I. Under the first head, the Secretary refers to the correspondence with the Minister of the Chilian Government, and with Professor G. B. Airy, of the Royal Observatory of Great Britain, contained in the Washington volume. He also states that the general correspondence consists of two hundred and eleven letters, all of which are on file, and all of which have received an answer when necessary.

II. Under the second head, the Secretary states that he availed himself of the opportunity afforded by the issue of the circular for the Providence meeting, to send to each member of the Association the amount of his indebtedness, — an innovation which the Secretary thinks will be justified by the propriety of the act, as well as by the financial result.

III. Under the third head it is to be stated, that fifteen hundred copies of the Washington volume have been printed. This volume consists of three hundred and seventy-two pages, with nine wood-cuts, and six large maps. The whole edition has involved an expense of one thousand dollars, or about sixty-seven cents *per copy*.

IV. Under the fourth head it is to be remarked, that much of the financial labor must in *fact*, as it does by *law*, fall upon the Permanent Secretary. *At* the Washington meeting, and *since* that meeting, down to the time when the circular for the present meeting was issued, the amount of five hundred and ninety-seven dollars and twenty-five cents was collected by the Secretary, in the form of assessments, and one hundred dollars and seventy-eight cents by the sale of publications. This has been expended for the purposes of the Association, as well as eight hundred dollars received directly from the Treasurer. The items, with vouchers, will be found in the register of the account current which the Secretary has opened with the Association.

Since the issue of the circular for the Providence meeting, down to August 21 (an interval of only one month), the Secretary has received assessments to the amount of eleven hundred and seventy-six dollars and fifty cents, or double the amount received during the preceding fifteen months. At the same time it remains to be stated, that there still exists a large sum of indebtedness to the Association; amounting, on a rough estimate, to five thousand dollars. The Association now numbers one thousand and twenty-three members. Three dollars from each would pour into the treasury three thousand and sixty-nine dollars annually. The Secretary proposes to issue, after a suitable interval, a second circular to those who remain delinquent, and then to execute the law contained in Rule 19 of the old Constitution, or Rule 23 of the new Constitution printed by the Committee.

If the eleven hundred and eighty-six dollars and fifty cents just collected be added to the six hundred and thirty dollars and fifty-eight cents which appears, by the accompanying report of the Treasurer, to be the previous balance in the treasury, it gives a sum of one thousand eight hundred and seventeen dollars and eight cents with which to begin another financial year.

The probable expenses for that year may be loosely estimated as follows: —

Salary of the Permanent Secretary, . . .	$ 300
Publication of the Providence volume, . .	1,000
Republication of the Cleveland volume, . .	700
Total, . . .	$ 2,000

The sum total, which is exclusive of thirty-seven and a half dollars just paid by order of the Standing Committee to Mr. Brainerd for lithographing and printing Blodget's map, and of the expenses of the Providence meeting, and all other incidentals accruing during the current year, exceeds by one hundred and eighty-three dollars the money in the treasury. But it is hoped, that, by pressing vigorously the work of collection, the Association may be saved from pecuniary embarrassment, without restricting its publications.

In conclusion, the Secretary requests that his report and accounts may be audited by a Sub-Committee of the Standing Committee.

Respectfully submitted by

JOSEPH LOVERING,
Permanent Secretary.

REPORT OF THE AUDITORS.

THIS certifies that we have this day examined the above account of the Permanent Secretary, comparing the credits with the Treasurer's account, and with the receipt-book of the Secretary, and the debits with the several vouchers, and find the whole correct, and the balance of one hundred and eleven dollars and ninety-six cents properly credited in the next account.

JOHN JOHNSTON, } Auditors.
JAMES HALL, }

Providence, August 24, 1855.

REPORT OF THE TREASURER.

Since the meeting at Washington in April, 1854, the Treasurer has received from G. P. Putnam thirty-five dollars; from S. F. Baird, one hundred and twenty-nine dollars and forty-eight cents; from J. M. Gilliss, one hundred and forty-two dollars and forty-seven cents; from assessments, ninety dollars and ninety cents: in all three hundred and ninety-seven dollars and eighty-five cents.

During the same time he has sent to Professor Lovering eight hundred dollars, and paid Brainerd and Burridge's bill for wood-cuts for the Cleveland volume, one hundred and nineteen dollars and seventy-five cents. Besides, there has been a discount of thirty-five cents for collecting.

The total amount, taken from the whole sum paid into the treasury since the Albany meeting, which is fifteen hundred and sixty-eight dollars and thirty-three cents, leaves in the hands of the Treasurer, at the commencement of the present meeting, six hundred and twenty dollars and fifty-eight cents.

A. L. ELWYN,
Treasurer.

VOTES OF THANKS.

Resolved, That the sincere thanks of the members of the Association be returned to the Trustees, President, and Professors of Brown University, for the cordial welcome which they have given, and for the ample accommodations afforded in the College buildings for the meetings of the Sections and Committees.

Resolved, That a set of the Volumes of the Proceedings of the Association be presented to the Library of Brown University.

Resolved, That the thanks of the Association be tendered to the Local Committee, and especially to its Chairman and Secretary, for their constant kindness and attention; and for their judicious and excellent arrangements both before and during the meeting.

Resolved, That the thanks of the Association be tendered to the numerous Societies, Libraries, and Manufacturing Establishments which have extended invitations to its members.

Resolved, That the thanks of the Association be tendered to the Benevolent Street Congregational Society, for the use of their church for the delivery of the Address of the retiring President, Professor J. D. Dana.

Resolved, That the thanks of the Association be tendered to the Railroad and Steamboat Companies who have offered free return tickets to members in attendance at this meeting.

Resolved, That the thanks of the members of the Association be tendered to the Citizens of Providence, whose private hospitality has been so freely and so munificently extended during our whole sojourn in their beautiful city.

Resolved, That the thanks of the Association be tendered to the Citizens of Providence, for the most noble and generous entertainment offered to its members on August 22.

Resolved, That a set of the Volumes of the Proceedings of the Association be presented to the Providence Athenæum.

REPORT OF REMARKS

OFFERED ON OCCASION OF THE

RESOLUTIONS INTRODUCED BY PROF. A. D. BACHE,

IN HONOR OF

THE HON. ABBOTT LAWRENCE.

Mr. Samuel B. Ruggles rose to second the resolutions. He concurred most cordially in the judicious and eloquent tribute paid by Professor Bache to the private virtues and public services of Mr. Lawrence, and would only advert, in addition, to the position he had occupied in respect to American science, as rendering the proposed expression of feeling by this Association peculiarly appropriate.

Mr. Lawrence was probably the most important of the many important links which bind this Association to the community around it. In truth, he was the very type of that great and generous portion of the American public, ready and willing, at all times, to lend their hands, and heads, and hearts, and purses to the support of science. Nay, more, Mr. Lawrence stood forth a living exponent of the wide-spread opinion now pervading the American mind, that collegiate education is radically defective. He saw and felt, what so many others are beginning to see and feel, that man was made to study Nature; that physical science has become a positive necessity in any enlightened scheme of instruction; that the hitherto undisputed sway of mere language, of quiddities and verbal subtilties, has passed away. In a word, in Abbott Lawrence the fundamental idea was made incarnate, that men, to be men, must study not only *words*, but *things*.

Mr. Ruggles said that, for one, he held the charge that the community in general was indifferent to the necessity of the highest culture, to be wholly unfounded. On the contrary, the public desired and demanded a more comprehensive education, one more suited to their best and highest interests and necessities; not a narrow teaching drawn from the mouldering cloisters of dark and bygone ages, but a full, fresh, living volume of instruction, both in science and in letters, enlarged, modernized, and adapted to the century and to the civilization in which we are actually existing.

Mr. Ruggles said it had been his good fortune frequently to hear from Mr. Lawrence full and animated expositions of his views on this all-important point,—and it was but due to his memory now to bear testimony to the strong, manly sense, the practical sagacity, the noble, ample patriotism, which he carried into the whole subject. His broad and enlightened vision had enabled him to discern in this young empire of ours a new people, placed by the Providence of God on a new and all but untrodden continent, here to build up a new world by the fullest development and best exertion of all their physical, intellectual, and moral powers. Taking such a view, how could Mr. Lawrence think any education sufficient or suitable which should be devoted all but exclusively to languages and metaphysics, and failed to include a thorough study of the vast material Universe, with all its varied and majestic concords, its mighty and beneficent powers and agencies? Convinced of the wretched short-comings in this respect of most of our existing colleges, Mr. Lawrence founded and endowed, on a scale of requisite amplitude, the Scientific School at Cambridge, bearing his honored name;—and there it will stand, for coming ages, and as long as the educational history of our country shall endure, the precursor and model of kindred establishments, to be scattered broadcast throughout our favored land. It is not for this Association to add to the large and honest renown of their lamented patron, associate, and friend. He has himself sown the seed of his own ample and glorious harvest; for where, in all the broad expanse of our continental Union, from the Atlantic to the Pacific, is there a community or hamlet so small or remote as not to know and pronounce the name of Abbott Lawrence as the most sagacious, patriotic, and munificent patron of American science?

Dr. Wayland could hardly let these resolutions pass without rising to say a single word. It so happened that Mr. Lawrence was kind enough to converse with him on this subject when he was organizing this School, and he was very much struck, as his friend Mr. Ruggles was also, with the largeness of his views, the clearness of his conceptions, and with the distinct knowledge that he had of what he was doing. He had a distinct object in view; his object was to commence an institution which should be a type of other institutions that should spread the blessings of science throughout our country in a way in which it had not before been spread abroad. He looked upon him (Mr. Lawrence) as a type, as Mr. Ruggles had said, of what was to be. He had set an example for men of wealth in this country. While Boston would always be proud of the name of Lawrence, there would be other Lawrences arising in New York, in Philadelphia, in all our large cities, — a train of men that would do honor to the country and to human nature. But however large this train might be, however noble and however magnanimous, they would all date from the name of Lawrence, — they would all be the Lawrences of this country. He believed that no honor which they could confer would be really adequate to the noble, high-minded, patriotic effort of this man, whom all felt honored by claiming as their fellow-citizen.

Professor Peirce would not have it thought, because none of the tributes which had been paid Mr. Lawrence were from his own State, that he was not appreciated at home. He had been universally beloved and respected in his own State. All his friends and neighbors felt him to be the source of more goodness in his native city than perhaps any other man ever was.

Professor Silliman, senior, as the oldest member of this Association, begged also to offer his word of tribute to Mr. Lawrence, whom he was proud to claim as an old and valued friend. Upwards of twenty years ago, and while modern geology had to contend with bitter religious prejudices, Abbott Lawrence headed the list of the intelligent citizens of Boston willing to examine the wonders of Nature, and adore the wisdom of the Creator as exhibited in his material creation. Mr. Lawrence had a devout but fearless spirit, and did not hesitate to read the revelations of God, as well in his works as in his Word; displaying in this, as in all his other traits, the breadth and freedom and liberality of his manly nature.

The Association then adopted the resolutions unanimously, the members all rising, and with evident emotion.

ACCOUNT OF THE

Dr. AMERICAN ASSOCIATION *in*

Freight on seven boxes from Washington,	$ 10.33
" " " Boston,	2.00
Paid for storing Proceedings,	1.25
Two packages from S. F. Baird,	2.62
Box " "	3.37
Printed notices of election,	2.00
Circulars for Providence meeting,	20.00
Postage on them,	10.50
Eclipse circulars,	3.75
Paper for Washington Volume,	336.38
Printing " "	444.60
Wood-cuts,	23.50
Redfield's Map,	15.00
Index,	5.00
Paid for publishing Paper on the Eclipse, in Gould's Journal,	43.94
Extra copies of Peirce's Address,	37.94
Binding them,	21.12
Envelopes for them,	2.00
Extra copies of Gould's Eulogy,	16.50
Binding them,	7.60
Permanent Secretary's salary,	300.00
Expenses to Washington,	67.50
Postage,	9.17
Balance to next account,	111.96
	$ 1,498.03

Cambridge, August 1, 1855.

PERMANENT SECRETARY.

Account with Joseph Lovering. Cr.

Subscriptions received at Washington, ...	$ 547.25
Subscriptions received afterwards, to July 1, 1855, . .	50.00
Received from T. Walker, ...	3.00
" from J. Bartlett, ...	5.68
" from Pottsville Association, ...	11.25
" from Putnam & Co., ...	80.85
" Salary from the Treasurer, ...	300.00
" Draft from the Treasurer, ...	500.00
	$1,498.03

STOCK ACCOUNT OF THE PERMANENT SECRETARY.

Volumes distributed or sold.

Volumes	I.	II.	III.	IV.	V.	VI.	VIII.
DELIVERED TO							
Jared Sparks, . .		*	*	*	*	*	
John McRae, .			*	*	*	*	
Mitchel King, . .				*	*	*	
H. I. Bowditch, .	*		*		*	*	
Isaac M'Conihe, .					*		
J. H. Gibbon, .						*	
Thomas Hill, . .	*			*	*		
Isaac Lea, . .						*	
James Hayward, .					*		
W. J. McAlpine, .						*	
F. Mauran, . .				*	*	*	
Brown University,‡	*		*	*	*	*	*
Boston Society of Natural History, . .							*
Boston Athenæum,							*
American Academy, .							*
Harvard College, .					*	*	*
Elwyn's order, . .	*	*	*	*	*	*	
S. P. Lathrop, .	*	*	*	*	*	*	
Pottsville Association,†	*	*	*	*	*	*	*
Ovid Plumb,† .							*
Robert Townsend,† .							*
F. H. Smith,† .							*
J. B. Holland,† . .							*
George Suckley,† .							*
N. K. Davis,† . .							*
New York State Library,†							*
E. P. Prentice,‡ . .						*	
W. F. Webster,‡ .							*
G. B. Paine,‡ . .							*
F. Paine,‡ . .							*
J. T. Caswell,‡ . .							*
Members, . .							383
G. P. Putnam & Co., .							21
Munroe & Co., .							25
Total, . .	6	4	7	9	13	14	446

† Sold. ‡ By order of the Association.

BALANCE OF STOCK.

Volumes	I.	II.	III.	IV.	V.	VI.	VIII.
Received from S. F. Baird, ...	86	32	292	274	494	353	1483*
Delivered to members or sold, . .	6	4	7	9	13	14	446
Remaining, . . .	80	28	285	265	481	339	1037
Received from E. N. Horsford, . .	4	1					
From Munroe & Co.,		25					
Balance, Mar. 20, 1856,	84	54	285	265	481	339	1037

ACCOUNT OF G. P. PUTNAM & Co. WITH THE ASSOCIATION.

Volumes on hand.

Volumes	I.	II.	III.	IV.	V.	VI.	VIII.	Value.
January 1, 1854, .	25	25	25	68	25	160		$ 442.50
January 1, 1855, .	18	18	18	72	18	160		$ 359.36
January 1, 1856, .	19	19	19	68	10	157	20	$ 374.38

Due to the Association for Volumes sold, ...	$ 68.15
Due to the Association for 21 copies of Volume VIII., .	31.50
	$ 99.65
Due from the Association for Volumes I. – VI. delivered to N. K. Davis, ...	7.51
	$ 92.14
Paid to J. Lovering's order, ...	80.85
Now due, ...	$ 11.29

March 20, 1856.

* These were received from the printer.

INDEX.